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ON THE DISTANCES OF THE FIXED STARS.

By A. C. RANYARD.

ONE of the first objections raised against the hypothesis of Copernicus as to the earth's motion round the sun was that the stars do not appear to move as they should if the earth moved. It was readily seen, even by objectors with very little geometrical training, that the earth's motion round the sun involved such an enormous displacement of the spectator's place during the course of the year that the stars ought to appear to shift their places, and the nearer stars would be seen to move with respect to more distant stars laying behind them, as the earth passed from one side to the other of its orbit. The genius of Copernicus had foreseen the objection, and had suggested the true answer. He attributed the absence of all sensible motion to the immense distances of the stars compared with the diameter of the earth's orbit. But this answer required for its comprehension too great an effort of the imagination from his opponents.

Even Tycho Brahé was staggered and unable to accept the otherwise attractive Copernican theory of the Universe—by reason of the strain which it put upon his imaginative powers. Tycho Brahé had, by a long series of observations with instruments more perfect than had hitherto been used, determined the places of the larger stars with a probable error of about a minute of arc, and he had satisfied himself that he could not detect with his instruments any annual displacement of the stars.* On the other hand, he estimated that stars of the first magnitude had an apparent diameter of two or three minutes; and he saw that if the Copernican system were true, this necessarily involved stars of the first magnitude exceeding in dimensions the whole amplitude of the earth's orbit, a conclusion which appeared to him to be palpably absurd and sufficient to justify the rejection of the Copernican hypothesis. The invention of the telescope showed that the apparent magnitudes which Tycho Brahé and other naked-eye observers had assumed to be the real diameter of the stars were to a great extent illusory. Galileo satisfied himself that the apparent diameter of α Lyræ did not exceed five seconds of arc,† and Horrocks proved that their diameter could only be a very small fraction of a second of arc. He observed, with his friend Crabtree, an occultation of the Pleiades by the moon on the evening of the 19th of March 1639; they both noted that the stars were obliterated almost, if not quite, instantaneously by the moon; and Horrocks remarked that, as the moon moves in the heavens through only about half a second of arc in a second of time, the real diameter of the stars must be a very small part of a second of arc.

Galileo was fully alive to the importance of this objection to the Copernican hypothesis, and felt convinced that some relative motion or parallax‡ of the stars would be observed. He proposed in the third of his dialogues on the Systems of the World that pairs of stars lying close together should be observed, a proposal which gets rid of complications more numerous than Galileo was aware of, and reduces the problem to one of careful micrometrical measurement. The same idea suggested itself to several subsequent astronomers, but it was not till this century that the parallax of a star§ was actually determined by this obviously simple method. The proposal, however, proved fruitful in another and unexpected manner. Sir William Herschel, in order to carry out his plan of sounding the heavens, set himself to collect suitable double stars which were, at that time, supposed to be merely optically double, because they laid near to the same line of sight. Though he did not succeed in finding any parallactic displacement, he was rewarded by the discovery of evidence in several cases of an actual physical connection between the stars, proving them to be binary systems in motion round a common centre of gravity.

I do not propose in this paper to enter at any length into the history of the early attempts to detect a parallax, or annual shift, of the places of fixed stars. The early history of such researches is admirably set out in the classical memoir of Dr. C. A. F. Peters, published in the volume of *Mémoires* of the St. Petersburg Academy for 1853. It brings the history of the research up to the year 1842. There is also a short *resumé* of the earlier attempts to obtain evidence of an annual parallax, in Prof. Robert Grant's *History of Physical Astronomy*. No reliable evidence of stellar parallax was obtained till this century. We will therefore confine our attention to investigations, the results of which have been published since the beginning of this century. Before proceeding to discuss such determinations, it may be well to point out that the discovery of Parallax is one of the rare triumphs of the deductive method.

* See the *Astronomiæ instauratæ progymnasmata*, 4to, Francfort, 1610, P.I., 684. Tycho Brahé imagined that his measures were correct to a third of a second of arc.

† *Opere de Galileo*, tome iv., p. 259.

‡ The parallax of a star is the angle which the mean radius of the earth's orbit about the sun would subtend as seen from the star's place. The greatest shift of the star's place due to the change of the observer's place as he is carried round the sun is equal to about double the parallax.

§ The parallax of α Lyræ from observations made in 1835–38 by W. Struve, and the parallax of 61 Cygni from observations made in 1837–38 by W. Bessel.

Most of the discoveries of Astronomy and Physical Science are fruits of the inductive method; that is, Phenomena have been observed and theories have been framed to account for them. Probably the discovery of Neptune from the perturbations of Uranus, and the conical refraction of light by biaxial crystals predicted from the equation to the wave surface are the two other most notable cases in which important additions have been made to our knowledge by the deductive method.

When the facts to be observed lie in the Border-land which limits our powers of perception, the inductive method is much the safest to pursue, for the new truths that are acquired by it are founded upon facts which have forced themselves upon our attention as being contrary to what might be expected. The mental process followed is generally to doubt such a fact, then to become convinced of its existence, and then to hit upon the true explanation.

On the other hand, the deductive method has not infrequently led to supposed confirmations of theory which have afterwards been found to be doubtful, and to other supposed confirmations which have subsequently been found to be explainable in quite a different manner. Thus the early history of the search for stellar parallax is a most interesting history of astronomical mares' nests.

One observer after another discovered evidence of an annual shift in star places, which was either not confirmed by others or was shown to be due to some other cause than parallax. One of the first of these was Christopher Rothmann, astronomer to the Landgrave William of Hesse, who found a difference of 1·5 minutes of arc between the latitude of the observatory of Cassel as determined in the summer and in the winter. He also thought that he had showed that the distances between certain stars varied as much as two minutes of arc in the course of the year; and he relied on these observations as proving the earth's motion round the sun. But Tycho Brahé, with superior instruments, was unable to confirm these observations, and he attributed the differences between Rothmann's summer and winter observations to variations in the instruments employed due to the differences of temperature.

The ingenious Robert Hooke thought to get rid of difficulties due to changes of refraction at different periods of the year by selecting for observation the star γ *Draconis*, which passed within a few minutes of the zenith of his observatory at Gresham College, in the City of London. From four zenith distances of the star as observed by him in the months of July, August, and October of the year 1669, he found a shift of 22 to 24 seconds in the place of the star,* which he concluded corresponded to an annual parallax of between 27″ and 30″. Twenty-six years later, Molyneux and Bradley, with a view of verifying the interesting result announced by Hooke, undertook a similar series of observations with a zenith sector. They soon found that the star had an apparent motion at right angles to the direction which should have resulted if the effect had been occasioned by parallax. The star continued to advance until it attained a distance of 20″ from its original position; it then turned, and, in the course of a year, returned again to its original position. At first Bradley thought that the phenomenon might be due to a nutation of the earth's axis; but he undertook a series of observations of other stars which soon showed that the changes could not be due to an annual shift in the direction of the earth's axis, or to an annual shift, as he had thought might be possible, in the direction of the plumb line. By a happy inspiration he ultimately recognised that the observed phenomena might be completely accounted for by the aberration of light caused by the earth's motion in its orbit, as compared with the velocity of propagation of the light emanating from the star.

Thus another important truth was inductively discovered whilst searching for confirmation of a truth which had been deductively surmised, but the evidence of which was still far beyond the range of accuracy attainable with the instruments then used. A star is displaced by aberration in the direction of the tangent to the earth's orbit, but it is displaced by parallax in the direction of the radius sector towards the sun. The displacement caused by aberration is very large compared with that caused by parallax. The earth, as it were, drives the star before it in the aberrational orbit, while parallax slightly shifts the star towards the sun's place. Thus the aberrational orbit of a star situated in the pole of the ecliptic is an ellipse of similar eccentricity to the earth's orbit, and about 41″ in diameter, while a star in the ecliptic is caused by aberration to oscillate backwards and forwards in a straight line 41″ long. These apparent motions have to be eliminated before the effect of parallax can be detected by comparing observations of the absolute places of a star. As the parallactic orbit is in no instance 2″ in diameter, the importance of properly eliminating the displacement due to aberration will be evident.* It is curious that till 1838 all the determinations of stellar parallax depended upon comparisons of the absolute places of stars, which involve not only the elimination of the apparent shift in the star's place due to aberration, but also corrections for the shifts due to nutation, precession, and the proper motion of the star, as well as sometimes a correction for irregularities discovered in the proper motion.

Since the beginning of this century (and notably in the half century which has nearly elapsed since 1842, the epoch at which Dr. Peters's survey of parallax work ceases), the determinations of stellar parallax have not been quite as contradictory as they previously were. The diagram given at the beginning of this paper is intended to bring before the mind, in a graphic form, the results which have been arrived at in the ninety years since the beginning of this century. The diagram has been prepared by Mr. Wesley and myself from the list of parallaxes collected by Mr. Sadler, which is given at the end of this paper.†

It will be seen that with the exception of α *Centauri* and 61 *Cygni* there are very few stars with respect to which there is any marked concurrence between the determinations of parallax obtained by different observers, and even in the case of α *Centauri* and 61 *Cygni* the determinations are far from grouping themselves within

* *An Attempt to prove the Motion of the Earth from Observations.* 4to. London, 1674.

* The amount of the aberrational displacement depends upon the velocity of the earth and the velocity of light: according to the latest determination of Newcomb and Michelson, the velocity of light is 186,330 miles $\pm$ 20 miles. The mean velocity of the earth in its orbit, if we assume the solar parallax to be 8″·8, is 18·50 miles. This makes the constant of aberration 20″·480, a quantity which will vary with every change in the adopted velocity of light, and in the adopted value of the earth's mean distance from the sun.

† Mr. Sadler's list of parallaxes was in type in July 1889, with the exception of those found by Dr. Kapteyn and the later ones determined by the Oxford observers, and it is chiefly owing to my delay in preparing the diagram and writing this paper that it has not appeared before. On the 13th of August Prof. Oudemans published in the *Astronomische Nachrichten*, 2915, a very similar but less complete list of parallaxes. Mr. Sadler's list contains 226 determinations of parallax referring to seventy-nine stars, while Prof. Oudeman's list refers to determinations of parallax of only fifty stars. My special thanks are due to Mr. Wesley for the time and care he has devoted to the preparation of the diagram.

the narrow limits which we might be led to expect from the estimates of the different observers of their own probable error.

This is probably due to the fact that observers have been endeavouring to measure quantities much smaller than they can see. According to the estimate of the clear-sighted Dawes, it requires a very perfect instrument and a very perfect night to permit an observer with a telescope of 4·56 inches aperture to divide a double star with a separation of a second of arc. Theoretically it would require a telescope of 45½ inches aperture (*i.e.* considerably larger than the Lick telescope) to separate a double star with a distance of a tenth of a second between its components. Practically the limit of actual division of star discs for such large telescopes has not advanced beyond a fifth of a second of arc. But many parallax observers using comparatively small instruments have been relying on their measures to much smaller fractions of a second. Thus Gill gives the probable error of his determination of the parallax of *α Centauri* with a 4¼-inch heliometer as ±0·013″, or about an eightieth part of a second of arc, with a telescope which, according to Dawes, would not divide a double star with a distance of two seconds. For the two halves of the object glass of such a heliometer would, for dividing purposes, be at most equivalent to two 2⅛-inch telescopes.

The mistake made by such observers is in assuming that the law of probable error will hold with regard to their guesses as accurately as it does with regard to reasonable estimates. It has been amply proved by Quetelet, Francis Galton, and others, that all physical quantities which vary in magnitude, such as the heights of individuals in a nation, or the weights of seeds, arrange themselves with respect to the mean height or mean weight according to the law of probable error; but no one has shown that a man's guesses will arrange themselves according to a curve of probable error. In fact, a little consideration will show that it is very improbable that they should be capable of arrangement in so regular a manner.

Let us suppose that a prisoner is placed in a marble rectangular cell, the length of which is told him to the thousandth of an inch, and he is required to measure the breadth of the cell to the thousandth of an inch with no other measuring instrument than his boots. He might, perhaps, after repeated measurements, by carefully adjusting his heel to his toe with about the same pressure each time, and by giving great attention to estimating the spaces left over after each pacing, succeed in measuring the breadth of the cell with a probable error of a twentieth part of the length of his boot, or even say a fiftieth part; but most people will at once feel satisfied that no amount of clever guessing would enable him to *measure* the breadth of the cell with a probable error of a thousandth of an inch. Yet the ordinary method of treating observations, if blindly used, would theoretically give a probable error for the result which would decrease in magnitude with the number of the measurements, and would ultimately be reduced below a thousandth of an inch.

The photographic method of determining parallax seems at first sight to offer some great advantages. It permits of opposite phases being directly compared side by side and measured over and over again; but the accuracy with which such measures can be made has, I believe, been greatly over-rated. With a focal length of 10 feet such as that of the reflector of the Oxford Observatory, a second of arc corresponds to about a 1600th of an inch, and a tenth of a second of arc to a 16000th of an inch. An ordinary sharp-sighted person can, without making use of any magnifying power, possibly measure down to the 200th of an inch. A power of 80 will therefore be necessary to make anything like satisfactory coincidences between a micrometer wire and definite marks, as, for example, the divisions on a scale of 16000ths of an inch. But the image of a star has no definite edge, and, with a power of much less than 80 diameters, photographs taken on ordinary dry plates show a very granular structure, so that a star image examined with only a moderately high power appears to break up into a number of definite dots or granules which increase in density towards the centre, and fade off gradually towards the edge of the image. The problem of measuring the distance between two such patches is somewhat like measuring the distance between two heaps of shingle, where the observer has not to measure the distance from the edge of one heap to the edge of the other, but from a point which he selects as the centre of one heap to a point which he selects as the centre of the other.

An examination of the diagram seems to indicate that there is only a concurrence between the parallax determinations of different observers which about corresponds to the magnitudes their instruments enable them to perceive. With the exceptions of Hall's measures with the Washington 26-inch telescope, all parallax work has been done with comparatively small instruments. Of the two stars whose distances seem to be best determined, 61 *Cygni* and *α Centauri*, there is a difference between the results of Hall and Auwers for 61 *Cygni* of more than three tenths of a second, which corresponds to an annual shift upon the heavens of more than six tenths of a second; and in the case of *α Centauri* there is a difference between the results of Henderson and Elkin-Maclear of more than half a second, which would correspond to a shift of more than a second upon the heavens. At greater distances similar differences give discordances in the distance which are even more striking. Man seems to be only able at present to measure the distance of stars with a parallax greater than the third of a second (that is with a distance of about ten light years), with a degree of accuracy which for the ordinary affairs of life would be thought rough even by the unskilled cook who measures her salt by the handful and her pepper by the pinch, and we are separated from the more distant stars by a gulf the breadth of which we can still only guess at.

LIST OF STELLAR PARALLAXES.

By HERBERT SADLER.

I HAVE read what Mr. Ranyard says on the subject of stellar parallaxes, and I perfectly agree with him; indeed, the only possible fault I could find is that he has understated some parts of his case.

This list of parallaxes is intended to include all published from 1800 to the end of 1889. In the list itself I give the ordinary designation of the star, the approximate places for 1890, the magnitude roughly to the nearest half magnitude in the photometric scale; the name of the observer or publisher of the determinations; the date, if marked with an asterisk the date of publication is to be understood; the method of observation; the aperture of the instrument employed; the reference to the place of publication, and the actual parallax and probable error as determined by the observer. It may be useful to remark that the distance in light years can be found by dividing 3·262 by the parallax in seconds.

β CASSIOPEIÆ. $0^h.\ 3{\cdot}3^m.+58°\ 33'$ (2½). PRITCHARD (1888), by means of photography, with a reflector of 13-inches aperture. Relative. (M.N. vol. xlix No. 1) $+0{\cdot}162'' \pm 0{\cdot}051''$. $+0{\cdot}154 \pm 0{\cdot}035''$ (Oxford University Observations, part iii.) (1887–1888).

34. GROOMBRIDGE. $0^h.\ 12{\cdot}2^m.+43°\ 24'$ (8). AUWERS (1863–1866), refractor of 4⅓-inches aperture. Absolute. (Abhandlungen der Akad. zu Berlin. 1867. Math. Cl. p. 18)+0·307″±0·0376″.

ζ TUCANÆ. $0^h.\ 14{\cdot}2^m.-65°\ 31'$ (4). ELKIN (1881–1882), heliometer of 4½-inches aperture. Relative. (Mem. R.A.S. vol. xlviii. part i.)+0·06″±0·02″.

α CASSIOPEIÆ. $0^h.\ 34{\cdot}3^m.+55°\ 56'$ (var.) PRITCHARD (1888), by means of photography with a reflector of 13-inches aperture. Relative. (M.N. vol. xlix. No. 1)+0·071″±0·040″.+0·036″±0·023″ (Oxford University Observations, part iii.) (1887-1888).

η CASSIOPEIÆ. $0^h.\ 42{\cdot}4^m.+57°\ 14'$ (3½). O. STRUVE (1856*), refractor of 15-inches aperture. Relative. (Bulletin physico-math. Ac. Sc. Pétersbourg, vol. xiv. p. 231)+0·154″±0·045″: (this is the mean of the results from measures of distance and position angle). SCHWEIZER-SOCOLOFF (1863–1866), refractor of 10¾-inches aperture. Relative. (Annales de l'Observatoire de Moscou, vol. viii. part ii. p. 89). +0·3743″±0·0723″ (p. 90+0·2750″±0·055″).

γ CASSIOPEIÆ. $0^h.\ 50{\cdot}1^m.+60°\ 7'$ (2¾). PRITCHARD (1888) by means of photography with a 13-inch reflector. Relative. (M.N. vol. xlix. No. 1)−0·14″±0·047″ and +0·007″±0·042″.−0·025″±0 031″ and +0·050″±0·036″ (Oxford University Observations, part iii.) (1887-1888.)

μ CASSIOPEIÆ. $1^h.\ 0{\cdot}7^m.+54°\ 23'$ (5). BESSEL (1816), transit instrument of 2¾-inches aperture. Relative. (Bessel Abhandlungen, vol. ii. p. 216)−0·12″±0·29″. O. STRUVE (1856*), refractor of 15-inches aperture. Relative. (Bulletin physico-math. Ac. Sc. Pétersbourg, vol. xiv. p. 231)+0·342″±0·052″. SCHWEIZER-SOCOLOFF (1863–1866), refractor of 10¾-inch aperture. Relative. (Annales de l'Observatoire de Moscou, vol. viii. part ii. p. 89)−0·1432″ ±0·1146″ (p. 90+0·0836″±0·060″ from angles of position only). PRITCHARD (1888), by means of photography with a 13-inch reflector. Relative. (M.N. vol. xlix. No. 1)+0·036″±0·025″.

NOVA ANDROMEDÆ. $0^h.\ 36{\cdot}7^m.+40°\ 40'$ (Nova). FRANZ (1885), heliometer of 6½-inches aperture. Relative. −0·32″+0·12″ (very doubtful). Another determination −0·11″±0·14″ (A.N. No. 2816). HALL (1885), refractor of 26-inch aperture. Relative. Parallax insensible.

α URSÆ MINORIS. $1^h.\ 18{\cdot}5^m.+88°\ 43'$ (2). PIAZZI-CACCIATORE (1802–1804), transit instrument of 3-inches aperture. Absolute. (Præcipuarum Stellarum inerrantium Pos. Med.)+1·31″. BRINKLEY (1809–1810), vertical circle of 4-inches aperture. Absolute. (C. A. F. Peters. Recueil de. Mém. des Astr. de Poulkova, vol. i. p. 38)+0·24″. VON LINDENAU (1818), from 890 observations by various astronomers. Absolute. (B.J. 1820)+0·144″+0·032″. BRIOSCHI (1819–1820), vertical circle of 4-inch aperture. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, vol. i. p. 175), +0·60″±0·12″. W. STRUVE (1818–1821), transit instrument of 4½-inches aperture. Absolute. (Peters *op. cit.*)+0·075″±0·034″. W. STRUVE and PREUSS (1822–1838), meridian circle of 4⅓-inches aperture. Right ascensions. Absolute. (Peters *op. cit.* p. 65) +0·172″±0·027″. W. STRUVE and PREUSS (1822–1838), as above. Declinations. Absolute. (Peters *op. cit.*)+0·147″±0·030″. C. A. F. PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (Peters *op. cit.* p. 136)+0·067″±0·012″. GLASENAPP (1880*), rediscussion of Peters's result. Absolute. (S. Glasenapp. Refraktsionnya oklony)+0·068″±0·012″. L. DE BALL (1885*), discussion of Wagner's observations, 1861-1872, with meridian circle of 6-inches aperture. Absolute. (A.N. No. 2,667)+0·015″±0·015″. PRITCHARD (1888), by means of photography with a 13-inch reflector. Relative. (M.N. vol. xlix. No. 1)+0·073″±0·014″.+0·079″ (Oxford University Observations, part iii.) (1887).

ε (LAC. 1060) ERIDANI. $3^h.\ 15{\cdot}5^m.-43°\ 29'$ (4½) ELKIN (1881–1883), heliometer of 4½-inches aperture. Relative. (Mem. R.A.S. vol. xlviii. part i.)+0·14″±0·02″.

P. iii. 242. $4^h.\ 1{\cdot}3^m.+37°\ 47'$ (6½) Sir R. BALL (1880–1881), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part v. p. 226)+0·045″±0·070″.

o² (40) ERIDANI. $4^h.\ 10{\cdot}2^m.-7°\ 48'$ (4½). GILL (1881–83), heliometer of 4½-inches aperture. Relative. (Mem. R.A.S. vol. xlviii. part i.)+0·166″±0·018″. HALL (1883–1884), refractor of 26-inches aperture. Relative. (Washington Observations for 1883. App. ii.) +0·223″±0·020″.

α TAURI. $4^h.\ 29{\cdot}6^m.+16°\ 17'$ (1). PIAZZI (1792–1804), vertical circle of 3-inches aperture. Absolute. (C. A. F. Peters. Recueil de Mém. des Astr. de Poulkova, vol. i. p. 30)+1·5″. O. STRUVE-SIDANOW (1850–57), refractor of 15-inches aperture. Relative. (M.N. vol. xliv. p. 237)+0·516″±0·057″. HALL (1886–1887), refractor of 26-inches aperture. Relative. (Gould's Astr. Journal. No. 156)+0·102″±0·0296″. ELKIN 1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale University Observatory) +0·116″±0·029″.

α AURIGÆ. $5^h.\ 8{\cdot}6^m.+45°\ 53'$ (0·2). PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 136)+0·046″±0·200″. O. STRUVE (1856*), refractor of 15-inches aperture. Relative. (Bulletin physicomath. Ac. Sc. Pétersbourg, vol. xiv. p. 234+0·305″±0·043″ (from measures of distance−0·087″±0·130″; from those of pos. angle+0·362″± 0·048″). GLASENAPP (1880*), rediscussion of Peters's result. Absolute. (Refraktsionnya oklony)+0·522″±0·181″. ELKIN (1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale University Observatory)+0·107″±0·047″.

α ORIONIS. $5^h.\ 49{\cdot}2^m.+7°\ 23'$ (1). ELKIN (1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale University Observatory)−0·009″±0·049″.

α ARGÛS (CANOPUS). $6^h.\ 21{\cdot}5^m.-52°\ 38'$ (−1½). ELKIN (1881–1883), heliometer of 4½-inches aperture. Relative. (Mem. R.A.S. vol. xlviii. part i.)+0·03″±0·030″.

ψ⁵ AURIGÆ (*Comes* of). $6^h.\ 38{\cdot}8^m.+43°\ 41'$ (8½). SCHUR (1883–1885), refractor of 5-inches aperture. Relative. (A.N. No. 2723) +0·111″±0·034″.

α CANIS MAG. (SIRIUS). $6^h.\ 40{\cdot}3^m.-16°\ 34'$ (−1½), PIAZZI (1792–1804), vertical circle of 3-inches aperture. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, p. 31)+4″. HENDERSON and MACLEAR (1832–1837), mural circle of 4-inches aperture. Absolute. (Mem. R.A.S. vol. xi. p. 248)+0·23″±0·10″. (Henderson, (1832–1833)+0·34″±0·11″; Maclear (1836–1837)+0·16″±0·09″.) PETERS-MACLEAR (1853*), rediscussion of Maclear's results. Absolute. (Peters *op. cit.* p. 64)+0·15″±0·09″. GYLDÉN-MACLEAR (1864*), rediscussion of Maclear's results. Absolute. (Bulletin Ac. Sc. Pétersbourg, vol. vii. p. 370)+0·193″±0·087″. BELOPOLSKY-WAGNER (1863–1870), meridian circle of 6-inches aperture. Absolute. (A.N. No. 2888)+0·43″±0·099″. ABBE-MACLEAR (1868*), rediscussion of Maclear's results. Absolute. (M.N. vol. xxviii. p. 6)+0·27″ ±0·10″. GILL (1881–1883), heliometer of 4¼-inches aperture. Relative. (Mem. R.A.S. vol. xlviii. part i.)+0·370″±0·09″. ELKIN (1881–1882), same instrument and method as above. Relative. (*Op. cit.*)+0·407″±0·018″.

51 HEV. CEPHEI. $6^h.\ 48{\cdot}8^m.+87°\ 13'$ (5). L. DE BALL (1885*), discussion of Wagner's observations 1861–1872, with meridian circle of 6-inches aperture. Absolute. (A.N. No. 2667)+0·027″±0·019″.

α GEMINORUM. $7^h.\ 27{\cdot}6^m.+32°\ 8'$ (1½). JOHNSON (1854–1855), heliometer of 7½-inches aperture. Relative. (Radcliffe Observations, vol. xvi. p. 20)+0·198″±0·062″.

α CANIS MINORIS. $7^h.\ 33{\cdot}5^m.+5°\ 29'$ (0·5). PIAZZI (1802–1804), vertical circle of 3-inches aperture. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, vol. i. p. 31)±3″. AUWERS (1861–1862), heliometer of 6½-inches aperture. Relative. (A.N. No. 1415) mean+0·240″±0·029″. WAGNER (1852–1882). meridian circle of 6-inches aperture. Relative. (Mem. de l'Acad. Imp. de St. Pétersbourg, vii. series, vol. 31, part 2)+0·299″±0·038″. ELKIN (1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale College Observatory) +0·266″±0·047″.

β GEMINORUM. $7^h.\ 38{\cdot}6^m.+28°\ 17'$. ELKIN (1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale College Observatory)+0·068″±0·047″.

ι URSÆ MAJORIS. $8^h.\ 51{\cdot}7^m.+48°\ 28'$ (3). PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, p. 136)+0·133″±0·106″. GLASENAPP (1880), rediscussion of Peters's results. Absolute. (Refraktsionnya oklony)+0·127″±0·106″.

10 URSÆ MAJ. $8^h.\ 53{\cdot}5^m.+42°\ 13'$ (4). BELOPOLSKY-WAGNER (1863–1870), meridian circle of 6-inches aperture. Absolute. (A.N. No. 2888)+0·20″±0·11″.

STRUVE 1321 prec. $9^h.\ 7{\cdot}2^m.+53°\ 9'$ (7½). KAPTEYN (1885–1887), meridian circle of 6½-inches aperture. Relative. (A.N. No. 2935) +0·087″±0·021″.

ℓ URSÆ MAJ. $9^h.\ 25{\cdot}6^m.+52°\ 11'$ (3). KAPTEYN (1885–1887), as above (*op. cit.*)+0·046″±0·029″.

LAL. 19022. $9^h.\ 36{\cdot}5^m.+43°\ 23'$ (8). KAPTEYN (1885-1887), as above (*op. cit.*)+0·072″±0·020″.

30 LEON. MIN. $9^h.\ 54{\cdot}7^m.+32°\ 28'$ (5). KAPTEYN (1885–1887). as above (*op. cit.*)+0·071″±0·028″.

α LEONIS. $10^h.\ 2{\cdot}5^m.+12°\ 30'$ (1½). ELKIN (1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale College Observatory)+0·093″±0·048″.

GROOMBRIDGE 1618. $10^h.\ 4{\cdot}7^m.+50°\ 1'$ (6½). Sir R. BALL (1879–1880), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part v. p. 197)+0·334″±0·036″. KAPTEYN (1885–1887), as above (*op. cit.*). Relative. +0·177″±0·023″.

GROOMBRIDGE 1646. $10^h.\ 21{\cdot}3^m.+49°\ 22'$ (6). KAPTEYN (1885–1887), meridian circle of 6⅓-inches aperture. Relative. (A.N. No. 2935)+0·109″±0·025″.

GROOMBRIDGE 1657. $10^h.\ 27{\cdot}1^m.+49°\ 45'$ (7). KAPTEYN (1885–1887), as above (*op. cit.*)+0·025″±0·028″.

LAL. 20670. $10^h.\ 38{\cdot}2^m.+47°\ 44'$ (8½). KAPTEYN (1885–1887), as above (*op. cit.*)−0·006″+0·028″.

β URSÆ MAJ. $10^h.\ 52{\cdot}2^m.+56°\ 58'$ (2½). KLINKERFUES (1873*), by the displacement of the lines of the spectrum as determined by Huggins. Absolute. (Nachrichten von der Gesells, &c. der Göttingen, 1873, No. 13, p. 357)+0·0102″ to +0·0114″.

α URSÆ MAJ. $10^h.\ 56{\cdot}9^m.+62°\ 21'$ (2). BRIOSCHI (1819–1820), vertical circle of 4-inches aperture. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, vol. i. p. 175)+1·22″ ± 0·44″.

LAL. 21185. $10^h.\ 57{\cdot}4^m.+36°\ 42'$ (7½). WINNECKE (1857–1858), heliometer of 6-inches aperture. Relative. (A.N. No. 1147)+0·511″ ± 0·015″. WINNECKE (1872), same method and telescope. Relative. (Publ. der Ast. Gesells, No. xi.)+0·501″±0·011″. KAPTEYN (1885–1887), as above (*op. cit.*). Relative. +0·434″±0·028″.

LAL. 21258. $11^h.\ 0{\cdot}0^m.+44°\ 4'$ (8½). AUWERS (1860–1862), heliometer of 6½-inches aperture. Relative. (M.N., vol. xxiii. p. 75) +0·262″ ± 0·011″ (Absolute+0·271″ ± 0·11″). KRUEGER (1864), heliometer of 6-inches aperture. Relative. (Acta Fennicæ Soc. Scientiarum, vol. vii. p. 390)+0·260″±0·020″. Kapteyn (1885–1887), as above (*op. cit.*)+0·167″±0·027″.

STRUVE 1516. $11^h.\ 8{\cdot}1^m.+74°\ 3'$ (7). BERBERICH (1841–1866), micrometrical measures by various observers. Relative. (A.N. No. 2624)+0·28″±0·04″. WINNECKE (1878–1880), cited by Berberich. Relative. +0·199″ ± 0·050″. L. DE BALL (1885–1886), refractor of 10-inches aperture. Relative. (Mem. Acad. Royal de Belgique, vol. xlix.)+0·104″ ± 0·008″.

ŒLTZ. ARG. 11677. $11^h.\ 14{\cdot}4^m.+66°\ 27'$ (9). GEELMUYDEN (1878–1879), refractor of 7-inches aperture. Relative. (A.N. No. 2287, *cf* also No. 2666)+0·272″ ± 0·038″.

STRUVE. 1561 seq. $11^h.\ 33{\cdot}0^m.+45°\ 43'$ (6). KAPTEYN (1885–1887), meridian circle of 6⅓-inches aperture. Relative. (A.N. No. 2935)+0·047″±0·026″.

GROOMBRIDGE 1822. $11^h.\ 39{\cdot}8^m.+48°\ 17'$ (8). KAPTEYN (1885–1887), as above (*op. cit.*)+0·018″±0·031″.

GROOMBRIDGE 1830. $11^h.\ 46{\cdot}6^m.+38°\ 31'$ (6½). PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (Recueil de Mém des Astr. de Poulkova, vol. i., p. 136)+0·226″±0·141″. FAYE (1847), differences of R.A. with a neighbouring star obtained with a refractor of 13-inches aperture. Relative. (Comptes Rendus, vol. xxv. p. 141)+1·058″. [Comptes Rendus (vol. xxiii. p. 1078)+1·085″ ± 0·065″ (Peters ± 0·044″).)] WICHMANN and SCHLÜTER (1846–1848), heliometer of 6½-inches aperture. Relative. (A.N. No. 610) +0·182″ ± 0·018″. O. STRUVE (1847–1849), refractor of 15-inches aperture. Relative. (Recueil de Mém. des Astr. de Poulkova i. p. 293)+0·034″ ± 0·031″. WICHMANN (1853*), rediscussion of Schlüter's and his own observations. Relative. (A.N. No. 844)+0·08″ ±0·027″; another determination + 0·135″ ± 0·013″; by another method+0·72″ ± 0·043″. PETERS (1854*), rediscussion of the observations of Schlüter and Wichmann (A.N. No. 866)+0·141″ ± 0·013″. Relative. (Absolute. 0·148″±0·013″.) WICHMANN, quoted by Auwers. (Monat der Akad zu Berlin, 1874, p. 591). Relative+ 0·114″ ± 0·019″. SCHLÜTER, quoted by Auwers. (Monat. der Akad zu Berlin, 1874, p. 591). Relative. +0·166″ ± 0·018″. JOHNSON (1853–1854), heliometer of 7½-inches aperture. Relative. (Radcliffe Observations, vol. xiv. p. xlviii.) + 0·033″ ± 0·028″. JOHNSON, rediscussion by Auwers. Relative. (*Loc. cit.*)+0·023″ ± 0·033″. BRUNNOW (1870), refractor of 11¾-inches aperture. Relative (Dunsink Observations, part ii. page 23)+0·089″ ± 0·017″. GLASENAPP (1880*), rediscussion of Peters's results. Absolute. (Refraktsionnya oklony)+0·262″ ± 0·141″. Kapteyn (1885–1887), as above (*op. cit.*)+0·161″±0·029″.

γ URSÆ MAJ. $11^h.\ 48{\cdot}0^m.+54°\ 18'$ (2½). BRINKLEY (1808–1820), vertical circle of 4-inches aperture. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, vol. i. p. 40)+0·39″. KLINKERFUES (1873*), by the displacement of the lines of the spectrum as determined by Huggins. Absolute. (Nachrichten von der Gesells, &c., der Göttingen, 1873, No. 13, p. 357)+0·0164″ to+0·0183″.

LAL. 22632. $11^h.\ 56{\cdot}9^m.+43°\ 41'$ (7). Kapteyn (1885–1887), meridian circle of 6⅓-inches aperture. Relative. (A.N. No. 2935) −0·039″±0·038″.

LAL. 22810. $12^h.\ 4{\cdot}1^m.+41°\ 51'$ (7½). Kapteyn (1885–1887), as above (*op. cit.*)+0·067″±0·030″.

δ URSÆ MAJ. $12^h.\ 10{\cdot}0^m.+57°\ 39'$ (2½). KLINKERFUES (1873*), as above (*op. cit.*)+0·0244″ to+0·0271″.

ι URSÆ MAJ. $12^h.\ 49{\cdot}2^m.+56°\ 33'$ (2). BRINKLEY (1808–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 40)+0·33″. BRIOSCHI (1819–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 175)+0·41″±0·35″. KLINKERFUES (1873*), as above (*op. cit.*)+0·0301″ to +0·0334″.

ζ URSÆ MAJ. $13^h.\ 19{\cdot}5^m.+55°\ 30'$ (2½). BRINKLEY (1808–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 40)+0·28″. KLINKERFUES (1873*), as above (*op. cit.*)+0·0429″ to+0·0477″.

α VIRGINIS. $13^h.\ 19{\cdot}4^m.-10°\ 35'$ (1). BRIOSCHI (1819–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 175)−0·42″±0·33″.

η URSÆ MAJ. $13^h.\ 43{\cdot}2^m.+49°\ 52'$ (2). BRINKLEY (1818–1822), vertical circle of 4-inches aperture). Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p 40)+0.095″.

β CENTAURI. $13^h.\ 56{\cdot}1^m.-59°\ 50'$ (1). MACLEAR (1839–1849), mural circle of 5-inches aperture. Absolute. (Mem. R. A. S. vol. xxi. p. 152)+0·470″±0·044″. MOESTA (1860–1864), transit circle of 6-inches aperture. Absolute. (A.N. No. 1688)+0·213″ ± 0·069″. MOESTA (1860–1864), a fresh determination. Absolute. (A.N. No. 2349)+0·173″±0·07″. GILL (1881–1883), heliometer of 4½-inches aperture. Relative. (Mem. R.A.S. vol. xlviii. part i.)− 0·018″±0·019″.

α BOOTIS. $14^h.\ 10{\cdot}6^m.+19°\ 45'$ (0·1). BRINKLEY (1808–1814), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 39)+1·1″±0·15″. BRINKLEY (1808–1820), as above. Absolute. (*Op. cit.* p. 40)+0·61″. BRINKLEY (1818–1822), as above. Absolute. (*Op. cit.* p. 42)+0·652″. PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (*Op. cit.* p. 136)+0·127″±0·074″. JOHNSON (1855–56), heliometer of 7½-inches aperture. Relative. (Radcliffe Observations, vol. xvi.)+ 0·169″±0·044″. GLASENAPP (1880*), rediscussion of Peters's results. Absolute. (S. Glasenapp. Refraktsionnya oklony)+0·210″±0·073″. ELKIN (1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale College Observatory)+0·018″±0·022″.

α CENTAURI. $14^h.\ 32{\cdot}1^m.-60°\ 23'$ (0·7). HENDERSON (1832–1833), mural circle of 4-inches aperture, transit of 5-inches aperture. Absolute. (Mem. R. A. S. vol. xi. p. 68)+1·14″±0·11″. HENDERSON-MACLEAR (1839–1840), two mural circles of 4-inches and 5-inches aperture. Absolute. (Mem. R. A. S. vol. xii. 370)+0·913″±0·064″. PETERS-MACLEAR, a rediscussion of Maclear's results. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, vol. i. p. 63) + 0·976″±0·064; with other corrections + 0·49″ (*loc. cit.*). MOESTA (1860–1864), transit circle of 6-inches aperture. Absolute. (A.N. No. 1688)+0·880″±0·068″. MOESTA (1860–1864), a fresh determination. Absolute. (A.N. No. 2349)+0·521″±0·066″. ELKIN-MACLEAR (1880*), rediscussion of Maclear's results. Absolute. (Über die Parallaxe von α Centauri)+0·512″±0·080″. GILL (1881–1882), heliometer of 4½-inches aperture. Relative. (Mem. R. A. S. vol. xlviii. part i.)+0·76″±0·013″. ELKIN (1881–1883), same instrument. Relative. (*Loc. cit.*)+0·676″±0·027″.

β URSÆ MIN. $14^h.\ 51{\cdot}0^m.+74°\ 36'$ (2). BRIOSCHI, vertical circle of 4-inches aperture. Absolute. (Peters, Recueil de Mém. des Astr. de Poulkova, p. 175)−0·38″±0·37″.

η HERCULIS. $16^h.\ 39{\cdot}1^m.+39°\ 8'$ (3½). BELOPOLSKY-WAGNER (1862–1872), transit circle of 6-inches aperture. Absolute. (A.N. No. 2888)+0·40″±0·072″.

α HERCULIS. $17^h.\ 9{\cdot}6^m.+14°\ 31'$ (var.). BESSEL (1806), reflector of 9-inches aperture. Relative. (Briefwechsel mit Olbers, vol. i. p. 47) + 0·586″. JACOB (1853, 1856–1857), refractor of 6¼-inches aperture. Relative. (Madras Observations, vol. vii. Appendix, p. 20)+0·061″±0·008″.

π HERCULIS. $17^h.\ 11{\cdot}2^m.+36°\ 56'$ (3½). BELOPOLSKY-WAGNER (1862–1871), meridian circle of 6-inches aperture. Absolute. (A.N. No. 2,888)+0·11″±0·063″.

α OPHIUCHI. $17^h.\ 29{\cdot}8^m.+12°\ 38'$ (2). BRINKLEY (1808–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova)+1·57″.

ν DRACONIS. $17^h.\ 30{\cdot}0^m.+55°\ 16'$ (5, 5). BELOPOLSKY-WAGNER (1862–1870), meridian circle of 6-inches aperture. Absolute. (A.N. No. 2,888), preceding star+0·32″±0·076″; following star +0·28″±0·088″.

17,415 ŒLTZ. ARG. $17^h.\ 37{\cdot}2^m.+68°\ 27'$ (9). KRUEGER (1861–1862), heliometer of 6-inches aperture. Relative. (Acta Fennicæ Soc. Scientiarum, vol. vii. p. 383)+0·247″±0·0211″. SCHWEIZER-SOCOLOFF (1863–1866), refractor of 10¾-inches aperture. Relative. (Annales de l'Observatoire de Moscou, vol. viii. part ii. p. 90) +0·1914″±0·030″.

γ DRACONIS. $17^h.\ 54{\cdot}1^m.+51°\ 30'$ (2½). BRINKLEY (1808–

XUM

1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 40) − 0·08″. BRINKLEY (1818–1822), as above. Absolute. (*Op. cit.*, p. 42) + 0·0704″. POND (1813–1814), mural circle of 5-inches aperture. Absolute. (Ph. Trans. vol. cvii. p. 170) + 0·05″. MAIN (1852–1859), reflex zenith tube of 5-inches aperture. Absolute. (Mem. R. A. S. vol. xxix. p. 189) − 0·242″. DOWNING (1857–1875), same instrument. Absolute (M.N. vol. xlii. p. 344) − 0·131″ ± 0·041″.

NEBULA HERSCHEL. iv. 37. G. C. 4373. 17ʰ. 58·6ᵐ. + 66° 38′ (neb.). BREDICHIN (1876), refractor of 10¾-inches aperture. Relative. (Annales de l'Observatoire de Moscou, vol. iii. part iv.) + 0·009″ ± 0·041″. BRUNNOW (1871–1872), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part iii. p. 6) + 0·047″ ± 0·030″.

70 OPHIUCHI. 17ʰ. 59·9ᵐ. + 2° 33′ (4). KRUEGER (1859), heliometer of 6-inches aperture. Relative. (A.N. No. 1,212) + 0·169″ ± 0·010″. KRUEGER (1861–1862), same instrument and method. Relative. (A.N. No. 1,403) + 0·162″ ± 0·007″.

δ URSÆ MINORIS. 18ʰ. 7·8ᵐ. + 86° 37′ (4½). W. STRUVE (1814–1821), transit of 4½-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 55) + 0·203″ ± 0·037″ L. DE BALL (1885*), discussion of Wagner's observations, 1861–1872, with meridian circle of 6-inches aperture. Absolute. (A.N. No. 2,669) + 0·034″ ± 0·017″.

α LYRÆ. 18ʰ. 33·2ᵐ. + 38° 41′ (0·2). CALANDRELLI (1805–1806), sector of 3-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 33) + 3·9″. BRINKLEY (1810*), vertical circle of 4-inches aperture. Absolute. (*Op. cit.* p. 38) + 1·26″. BRINKLEY (1808-1817), as above. Absolute. (*Op. cit.* p. 39) + 0·66″ ± 0·08″. BRINKLEY (1808–1820), as above. Absolute. (*Op. cit.* p. 40) + 1·21″. BRINKLEY (1818–1822), as above. Absolute. (*Op. cit.* p. 41) + 1·138″. BRIOSCHI (1819–1820), vertical circle of 4-inches aperture. Absolute. (*Op. cit.* p. 175) + 0·37″ ± 0·29″. POND (1817*), transit instrument of 5-inches aperture, mural circle of 4 inches. (Ph. Trans. vol. cvii. p. 170) + 0·007″. AIRY (1836), mural circle (Troughton) of 4-inches aperture. Absolute. (Mem. R. A. S. vol. x.) + 0·224″. AIRY (1836), mural circle (Jones) of 4-inches aperture. Absolute. (*Loc. cit.*) − 0·092″. W. STRUVE (1835–1838), refractor of 9½-inches aperture. Relative. (Additamentum in Mens. Micr.) + 0·262″ ± 0·025″. PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 136) + 0·103″ ± 0·053″. O. STRUVE (1851–1853), refractor of 15-inches aperture. Relative. (Mem. Acad. St. Pétersbourg, series vii. vol. i. p. 50) + 0·147″ ± 0·009″. Absolute. + 0·137″. JOHNSON (1854–1855), heliometer of 7½-inches aperture. Relative. (Radcliffe Observations, vol. xvi. p. xxx.) + 0·154″ ± 0·046″. BRUNNOW (1868–1869), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part i. p. 29) + 0·212″ ± 0·010″. BRUNNOW (1870–1871), as above. Relative. (Dunsink Observations, part ii. p. 55) + 0·131″ ± 0·033″ (p. 57 + 0·188″ ± 0·033″). HALL (1880–1881), refractor of 26-inches aperture. Relative. (Washington Observations for 1883. Appendix ii.) + 0·134″ ± 0·006″. GLASENAPP (1880*), rediscussion of Peters's results. Absolute. (Refraktsionnya oklony) + 0·110″ ± 0·050″. ELKIN (1887–1888), heliometer of 6-inches aperture. Relative. (Report of Yale College Observatory) + 0·034″ ± 0·045″.

ŒLTZ. ARG. 18609. 18ʰ. 41·7ᵐ. + 59° 28′ (8). LAMP (1883–1885), refractor of 8-inches aperture. Relative. (A.N. No. 2,676) + 0·34″ ± 0·034″. LAMP (1885–1887), as above. Relative. (A.N. No. 2,808) + 0·353″ ± 0·014″.

CYGNI 6 (BODE). 19ʰ. 9·3ᵐ. + 49° 39′ (6). Sir R. BALL (1880–1881), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part v. p. 238) + 0·482″ ± 0·054″. HALL (1883–1886), refractor of 26-inches aperture. Relative. (Washington Astronomical Observations, 1883. App. ii. p. 42) − 0·021″ ± 0·008″.

δ AQUILÆ. 19ʰ. 20·0ᵐ. + 2° 54′ (3½). BRINKLEY (1808–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 40) + 3·20″.

σ DRACONIS. 19ʰ. 32·5ᵐ. + 69° 28′ (5). BRUNNOW (1868–1869), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part i. p. 44) + 0·222″ ± 0·022″. BRUNNOW (1869–1870), as above. Relative. (Dunsink Observations, part ii. p. 30) + 0·262″ ± 0·020″.

γ AQUILÆ. 19ʰ. 41·0ᵐ. + 10° 21′ (3). BRINKLEY (1808–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 40) + 2·19″.

α AQUILÆ. 19ʰ. 45·4ᵐ. + 8° 35′ (1). BRINKLEY (1808–1817), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 39) + 2·53″. BRINKLEY (1808–1820), as above. Absolute. (*Op. cit.* p. 40) + 1·57″. BRINKLEY (1818–1822), as above. Absolute. (*Op. cit.* p. 42) + 1·731″. TAYLOR (1834), mural circle of 3¾ inches aperture. Absolute. (Madras Observations, vol. ii. p. 132) + 0·978″. W. STRUVE (?) quoted by Elkin (Report of the Yale University Observatory) + 0·181″ ± 0·094″. ELKIN (1877–1878), heliometer of 6-inches aperture. Relative. (*Op. cit.*) + 0·199″ ± 0·047″.

β AQUILÆ. 19ʰ. 49 9ᵐ. + 6° 8′ (4). BRINKLEY (1808–1820), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 40) + 2·36″.

α CYGNI. 20ʰ. 37·7ᵐ. + 44° 53′ (1½). BRINKLEY (1810–1817), vertical circle of 4-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 39) + 0·78″. BRINKLEY (1808–1820), as above. Absolute. (*Op. cit.* p. 40) + 0·33″. BRINKLEY (1818–1822), as above. Absolute. (*Op. cit.* p. 42) + 0·50″. PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 136) − 0·082″ ± 0·043″. GLASENAPP (1880*), rediscussion of Peters's results. Absolute. (Refraktsionnya oklony) − 0·059″ ± 0·043″. ELKIN (1887–1888), heliometer of 6-inches aperture. Absolute. (Report of the Yale Observatory) − 0·042″ ± 0·047″.

61 CYGNI. 21ʰ. 2·0ᵐ. + 38° 13′ (5). ARAGO et MATHIEU (1812), repeating circle of (?) inches aperture (no details are given). Absolute. (Annuaire du Bureau des Longitudes, 1834. p. 281) + 0·55″. Baron von LINDENAU (1812–1814), transit instrument of 3¾-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 48), reduced anew by Peters, + 0·47″ ± 0·51″. Von Lindenau himself found no trace of parallax. BESSEL (1815–1816), transit instrument of 2¾-inches aperture. Absolute. (Peters, *loc. cit.*) − 0·88″ ± 0·19″. BESSEL and SCHLÜTER (1837–1840), heliometer of 6½-inches aperture. Relative. (Peters, *op. cit.* p. 60) + 0·3483″ ± 0·009″. (Bessel from the first series (1837–1838) found + 0·3136″ ± 0·014″.) PETERS (1842–1843), vertical circle of 6-inches aperture. Absolute. (Peters, *op. cit.* p. 136) + 0·349″ ± 0·080″. JOHNSON (1852–1853), heliometer of 7½-inches aperture. Relative. (Radcliffe Observations, vol. xiv. p. xxxix.) + 0·402″ ± 0·016″. O. STRUVE (1853), refractor of 15-inches aperture. Relative. (St. Pétersbourg Acad. Mém. vol. vii. p. 51) + 0·506″ ± 0·028″. (Absolute. + 0·493″). C. A. F. PETERS (1854*), rediscussion of Bessel's and Schlüter's results. Relative. (A.N. No. 866.) + 0·360″ ± 0·012″. AUWERS (1860–1862), heliometer of 6½-inches aperture. Relative. (M.N. vol. xxiii. p. 75) + 0·566″ ± 0·016.″ BELOPOLSKY-WAGNER (1862–1870), meridian circle of 6-inches aperture. Absolute. (A.N. No. 2888) mean + 0·525″ ± 0·093″. SCHWEIZER-SOCOLOFF (1863–1866), refractor of 10¾-inches aperture. Relative. (Annales de l'Observatoire de Moscou, vol. viii. part ii. p. 90.) + 0·4330″ ± 0·2091″. Sir R. BALL (18 7–1878), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part iii. p. 27) + 0·465″ ± 0·049″. Sir R BALL (1878), as above. Relative. (Dunsink Observations, part v. p. 166) + 0·468″ ± 0·032″. GLASENAPP (1880*), rediscussion of Peters's results. Absolute. (Refraktsionnya oklony) + 0·430″ ± 0·049″. HALL (1880–1886), refractor of 26-inches aperture. Relative. (Washington Observations, 1883. App. ii.) + 0·270″ ± 0·010″. PRITCHARD (1886–1887), by means of photography with a reflector of 13-inches aperture. Relative. (M.N. vol. xlvii. p. 445) mean + 0·432″. + 0·437″ (1886–1887), same method and instrument. (Oxford University Observations, part iii.)

α CEPHEI. 21ʰ. 16·0ᵐ. + 62° 7′ (2½). PRITCHARD (1887–1888), by means of photography with a reflector of 13-inches aperture. Relative. (Oxford University Observations, part iii.) + 0·061″.

ε INDI. 21ʰ. 54·9ᵐ. − 57° 14′ (5). GILL (1881–1882), heliometer of 4½-inches aperture. Relative. (Mem. R. A. S. vol. xlviii. part i.) + 0·27″ ± 0·02″. ELKIN (1881–1882), same instrument and method. Relative. (*Op. cit.*) + 0·12″ ± 0·19″.

LAC. 9352. 22ʰ. 58·8ᵐ. − 36° 28′ (7). GILL (1881–1883), heliometer of 4½-inches aperture. Relative. (Mem. R. A. S. vol. xlviii. part i.) + 0·285″ ± 0·02″.

BRADLEY 3077. 23ʰ. 8·0ᵐ. + 56° 34′ (5½). BRUNNOW (1870), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part ii. p. 49) + 0·069″ ± 0·027″. BACKLUND-WAGNER (1870–1873), transit circle of 6-inches aperture. Absolute. (Copernicus, vol. ii. p. 202) + 0·205″ ± 0·079″. GYLDÉN (1878–1881), refractor of 7-inches aperture. Relative. (Stockholm Akad Ofversigt 1882) + 0·283″ ± 0·047″.

85 PEGASI. 23ʰ. 56·4ᵐ. + 26° 30′ (6). BRUNNOW (1869–1870), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part ii. p. 42) + 0·054″ ± 0·019″.

THE BRITISH MUSEUM.

Mr. Louis Fagan, of the Prints and Drawings Department, British Museum, has arranged to deliver this month, at the Steinway Hall, near to Portman Square, a course of three lectures upon the treasures preserved in the National Collection. These lectures will deal, in a popular manner, with the foundation and progress of the Museum, its Egyptian, Assyrian, Babylonian, Greek, and Græco-Roman antiquities, the Library, Manuscripts, and Prints.

THE COMMON FLEA.—III.

By E. A. BUTLER.

THE muscular strength of the flea, like that of insects in general, is, *in proportion to its size*, exceedingly great. In instituting numerical comparisons, however, between it and the higher animals, by way of illustrating this fact, there are many points not altogether obvious at first sight which need to be considered before we can be satisfied that we have got results that are reliable, and not merely exaggerated and sensational statements devoid of any scientific value. For example, a flea is said to be able to leap to the height of a foot, which, taking the insect as a large female, is about 100 times its own length; and it is commonly assumed that this is a stupendous feat, corresponding in magnitude to what we should get if, for the sake of comparison, we were to imagine larger animals, such as the vertebrates, similarly constituted and endowed with powers enabling them in all cases to perform gigantic leaps to similar proportionate heights; on such a showing, this flea's leap certainly would be a marvel of muscular effort, for it would be as if an ordinary-sized man, supposed to be constituted like a flea, were able to take a vertical bound into the air which would carry him to a height greater than that of the top of St. Paul's!

A popular writer, taking a more moderate estimate of a flea's capabilities, says: "Perhaps we have not reflected that the average jump of a flea is about thirty times its own height, and that, supposing a man of six feet in stature were to perform the same leap, he would jump as high as the gallery of the Monument." Now such an assumption is altogether erroneous, and a leap of that height would not by any means represent a similar muscular efficiency, but, on the other hand, a far greater one—in other words, it would not be "the same leap," but one immensely in excess. The problem is, in fact, not quite so simple as it seems at first sight. Suppose we consider the work done. The flea raises its own mass against its own weight through the height of a foot (taking our former estimate of the maximum leap); if a man leapt up, say, only through *the same height*, he also would raise his mass against his weight through the height of a foot, and therefore the work done in the two instances would be proportional to the weights of the two animals. Next arises the question of the energy available for doing this work; this, on the supposition of the two animals being similarly constituted, and having muscles of a similar character, will be proportional to the volumes of the muscles, and therefore to their weights, which, again, in animals similarly constituted, would be as the weights of the animals themselves. The work done, therefore, being in the proportion of the weights, and the energy available for doing it being also in the same proportion, it follows that a leap through *equal* distances would represent equal muscular efficiency. And hence, on the above suppositions, a man's leap of a single foot, instead of something like 500 feet, would be more strictly comparable with a flea's leap of 1 foot. The height of the leap, therefore, does not by itself indicate any great superiority in relative muscular strength on the part of the flea.

But the question may be looked at in another light. Plateau has recently carried out some investigations as to the muscular strength of different insects by determining the maximum weight that they can lift; and though the flea itself does not happen to have been one of the insects experimented upon, yet the results obtained will tend to throw great light upon our subject. The *modus operandi* was as follows: a narrow groove was lined with cloth in order to give foothold, and the insect to be tested was placed in the groove. One end of a delicate thread was then attached to its body, and the other passed over a small pulley at the end of the groove. To the free end of the thread was attached a small pan, and sand was put into this till the insect could no longer raise it. The weight of the maximum load having been determined, as well as that of the insect itself, the ratio of the weight lifted to the weight of the insect's body could at once be calculated. By experimenting in this way with several well-known insects, Plateau established some very curious results, which at first sight seem rather paradoxical. He found that, the smaller the insect, the stronger relatively it becomes, and that in every instance the strength is proportionately far greater than that of vertebrate animals. Thus a hive-bee, weighing ·09 gramme, was found to be able to lift a weight equal to 23½ times that of its own body; while a large humble bee, weighing more than four times as much, had a relative muscular force only a little more than half as great, or, more exactly, could lift only 14·9 times the weight of its body. Similarly a large cockchafer, weighing ·94 gramme, could not raise more than 14·3 times its own weight; while a much smaller, but allied species, weighing only ·153 gramme, *i.e.* about $\frac{1}{6}$th part as much, was strong enough to raise 24·3 times its own weight, or more than half as much again as its lumbering relative.

If now we compare these results with those derived from the higher animals, we find that, while insects can raise from about 14 to 23½ times their own weight, the muscular force of a man or a horse, when expressed in the same way, are represented by the ridiculously small numbers ·86 and ·53 respectively. This high relative muscular force of insects, however, is not due to any superiority in the quality of their muscles, but is simply a direct consequence of their small size. For, with muscles similarly constituted, the contractile force depends, of course, on the number of muscular fibres, *i.e.* it varies as the cross section of the muscles, and is therefore proportional to the squares of linear dimensions; the weight, on the other hand, depends on the volume, and is therefore proportional to the cubes of linear dimensions. Hence the ratio of the contractile force to the weight decreases rapidly as the animal increases in size, or, in other words, the smaller the animal, the stronger, relatively, it must become, by virtue of that very decrease in size. While, therefore, it is quite true that, considering their size, insects are very much stronger than human beings, horses, or other large animals, such advantage does not necessarily proceed from any superiority in the quality of their muscles, but simply from their being so much smaller. And further than this, if their muscles were really as good as those of vertebrates, they ought, in accordance with the above calculation to be far stronger than they are. Their "absolute muscular force," indeed, as Plateau terms it, is low in comparison with that of higher animals, although their "relative muscular force" is extremely high. So, then, if we were to magnify the dimensions of our flea, without altering the character and quality of its muscular fibres, we should gradually lessen its relative strength, and, by the time it had reached human proportions, it would have turned out to be a far inferior animal. This somewhat complex subject may be found more fully discussed in Professors Miall and Denny's excellent work on the "Cockroach," from which the above illustrations are in substance derived, and to which those who wish to pursue the subject further are referred.

XUM

In this connection, a passing reference may be made to the so-called educated or performing fleas, which have sometimes been exhibited in different places. The chief difficulty met with in the training of fleas is, it seems, to restrain them from jumping, and to induce them to walk in an even and regular manner. One of the methods of overcoming this tendency to sprightliness, is to imprison them in circular glass-topped boxes which are kept revolving; every leap they take brings retribution in the shape of violent collision with the sides of their cell; and, as they are at the same time dazed, it is believed, by the movement of the box, they seem to get tired of this unpleasant experience, and, after a while, cease leaping, and settle down to a steady walk. Mr. W. H. Dall, an American entomologist, who once visited an exhibition of performing fleas with the view of determining to what extent the performances were really the result of training, and how far, therefore, they indicated any docility in the performers, came to the conclusion that all the movements consisted of struggles on the part of the insects to escape, and that what looked like concerted action, in which, of course, the greatest amount of intelligence seemed to be exhibited, could be accounted for in other and more natural ways. Take, for example, the waltzing, in which the fleas go spinning round in pairs to the sounds of a musical box. Two fleas of equal size and strength are attached to an extremely delicate piece of wire, one at each end; but as they are fastened in such a way as to face in opposite directions and at right angles to the wire, their struggles produce equal and opposite pulls at the end of the bar, or, in other words, form what in the language of mechanics would be termed a "couple," and therefore necessarily produce, without any intention on their part, a rotatory motion. To aid in the illusion, a small orchestra is added, consisting of fleas fastened before tiny models of musical instruments. As they are set upright, their legs can only flourish about in the air, suggesting the idea of their performing on the instruments; and if they should be at all slow to begin their "pawings," an attendant stirs them up by running a little barb from a feather across their legs, when, of course, they set to work kicking about vigorously. In the duelling performance we have something very similar to the orchestra: two fleas are fastened upright to little wire pillars, and tiny wands in lieu of swords attached to their fore-legs; they are placed opposite one another, but at such a distance that they are just out of one another's reach except with the "swords." As they brandish their legs about in their efforts to liberate themselves from their constrained position, it will occasionally happen that their "swords" will meet and produce the semblance of the clashing of weapons in a combat. Frank Buckland, in the account he gives of performing fleas, speaks of the supply as coming chiefly from elderly females (!), and of the price as ranging from 3d. a dozen in summer to 6d. in winter. He also states that the best fleas for this purpose are obtained from Russia, whence they are sent in pill-boxes packed in cotton wool.

The common flea is cosmopolitan in distribution; not so, however, that far more formidable but allied pest, the Chigoe or Jigger (*Sarcopsylla penetrans*) of tropical America. This villainous insect (Fig. 8), a short notice of which may appropriately close this paper, is something like a small flea, and is particularly noteworthy for two peculiarities, viz. the enormous size to which the abdomen of the female swells, by reason of the development of the eggs, and the marvellous habit it has of burrowing beneath the skin of its victims, thereby producing intense pain, ulcerations, and even sometimes death. It is only the female that thus burrows; after impregnation, she seeks the foot of a suitable host, and by means of her powerful mandibular lancets perforates the skin obliquely, usually beneath the toe-nail, and works herself under the surface

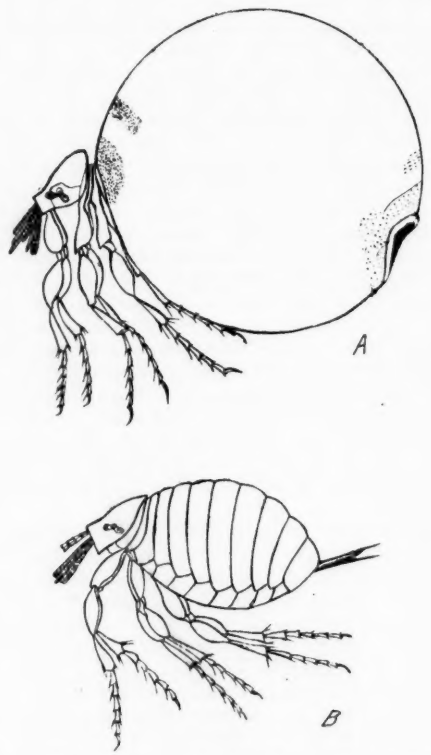


Fig. 8.—The Chigoe, or Jigger (*Sarcopsylla penetrans*). A. Female. B. Male.

till the tip of the abdomen only is visible. While she is in this position, the eggs, which are said to be as many as a hundred in number, advance towards maturity, and the body of the insect now swells to a large globular form, the head and thorax, of course, still retaining their original diminutive proportions; the increase in size continues till the eggs are ready for laying, when the abdomen is about the size of a small pea. After so enormous a dislocation of the abdominal viscera, it is perhaps not surprising that the deposition of the eggs should be the supreme effort of the insect. From the time of fecundation to that of oviposition, it lives solely to develop its progeny, and when the eggs are laid it perishes, leaving in the wound which has formed its resting-place only a shrivelled skin, which soon falls away.

The larvæ are of similar habits to those of our own flea; they live on sandy shores, often in great numbers; and the perfect insects seem also to be normally found in such situations, and their occurrence in houses may perhaps be accounted for by the fact that they do not confine their attention to the human species, but attack various other animals as well. Hence, dogs and mice, both of which are attacked by chigoes in the feet in the same way as man, may be the means of introducing them. This intolerable pest, the worst of all human parasites, has unfortunately been introduced into Africa by commercial intercourse with its head-quarters, but it seems unable to spread to districts beyond the tropics.

NEW COMET IDENTIFIED.

MR. E. E. BARNARD has forwarded us the following interesting paragraph, which is published in *The San Francisco Chronicle*:—

Mount Hamilton, Dec. 7.—News has been received at the Lick Observatory of a remarkable and extremely important theoretical discovery in cometary astronomy made by S. C. Chandler of Cambridge, Mass. Upon investigating some peculiarities in the orbit of the comet discovered last July by Mr. Brooks, and which is still under observation, Mr. Chandler found that it had made a remarkably close approach to the planet Jupiter in 1886, and that its path had then been entirely changed, the comet being thrown into its present small ellipse within a period of seven years.

Stimulated by this to find out what the path of the comet was previously to 1886, he was led to the discovery that it was identical with the famous Lexell's "lost comet" of 1770. The comet of 1770 was found by Lexell to be revolving in an orbit whose perihelion was near Venus and the aphelion near Jupiter, with a periodic time of five and a half years. The shortness of this period was singular, as the comet had never been seen previously to 1770, though in that year it was visible to the naked eye.

A further investigation, however, showed Lexell that in 1767 the comet had made a singularly close approach to Jupiter, at which time the great attraction of that planet caused a complete change in the comet's orbit, it being thrown into the small ellipse of 1770. Previously to this approach to Jupiter in 1767 the comet was moving in a very large orbit, with the perihelion near Jupiter and its aphelion beyond the orbit of Saturn. In this orbit the comet was always too distant to be seen from the earth.

In 1779 the comet again passed still closer to Jupiter and the attraction of the planet, outweighing that of the sun by over two hundred times, kept the comet under its influence for a number of months and again completely transformed the orbit, the perihelion distance once more becoming so great that the comet could not be seen from the earth.

At its appearance in 1770 this comet approached within less than 1,500,000 miles of the earth, a remarkably close approach—so much so, indeed, that La Place calculated that if the comet's mass had been equal to that of the earth it would have seriously affected the motion of the earth and would have lengthened our year by about three hours. As no sensible disturbance was experienced, La Place concluded that the mass of the comet must have been less than one three-thousandth part of the mass of the earth. It was doubtless vastly smaller than that.

The comet at its present return has attracted a widespread interest through the discovery at the Lick Observatory of the small companion comets that attend it in its motion through space. Mr. Chandler suggests that these may owe their existence to the encounter with Jupiter in 1886, the unequal strain from the opposing attractions of the planet and its satellite system having probably caused a disruption of the cometary matter sufficient to produce the companions.

Carrying the investigation back to 1886, Mr. Chandler found that the comet remained under the influence of the planet for more than eight months, and that previously to then it was revolving about the sun in twenty-seven years' time in an orbit whose perihelion lay near Jupiter. He also deduced the path that the comet moved in about the planet at the appulse, and found it to be an hyperbola, with an eccentricity very slightly greater than unity, so that "the comet narrowly escaped being drawn into a closed orbit as a satellite of Jupiter." The nearest approach of the planet in this path was on May 20, 1886, the distance being about 900,000 miles.

At present we cannot tell what have been the changes and vicissitudes of this comet since 1770, through the disturbing actions of Jupiter. It is now, however, free from the influence of that planet until 1921, when it will again suffer great perturbation that may once more whisk it out of sight for one hundred years or more. It will in the meantime return under favourable circumstances of observation in 1896, 1903, 1910, and 1917.

It is hardly possible to estimate the importance of the discovery of Mr. Chandler, as it presents to us one of the greatest physical and mathematical problems that astronomers have had to deal with. E. E. BARNARD.

CONCHOLOGICAL NOTES.

By S. PACE.

"THE OPERCULUM."

MOST spiral *gastropods* close the entrance to their shells with a kind of movable trap-door, technically known as the *operculum*, which, when present, usually fits the aperture with great accuracy, thus acting as a valuable safeguard against intruding enemies; but sometimes, especially in shells having large mouths, it is quite rudimentary.

None of the *Pulmonifera* (or snails) have a true operculum, though several, as we shall see presently, can, under certain conditions, form a *temporary* structure answering the same purpose; and in the one genus *Clausilia* there is, when adult, a permanent analogous (but not homologous) structure.

The operculum, when present, is always situated on the dorsal side of the *meta-podium* (or posterior portion of the so-called foot), being usually sessile* on its surface, as in *Buccinum* (Fig. 1); but sometimes, as in *Strombus* (Fig. 2),

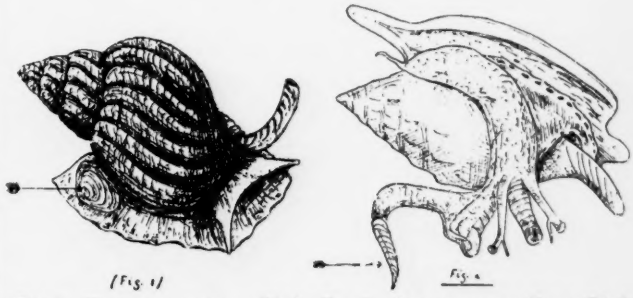


FIG. 1.—BUCCINUM UNDATUM (Lin.) Sessile operculum.

FIG. 2.—STROMBUS AURIS-DIANA (Lin.), Stalked operculum.

the surface to which it is attached is enormously extended, forming a muscular pillar on whose point the operculum is carried. It is attached to the animal by means of the *muscula columellaris*, a muscle which arises on the inner side of the mouth of the shell, and is inserted in the foot, sending off branches to the operculum and tentacles. The point of attachment between the operculum and muscle is shown by a scar on the inside of the former.

The different varieties of opercula can be roughly divided into two groups, namely the *lamellar* (Fig. 3 *a-e*),

* Without a stalk.

and *spiral* (Fig. 3 *g h*). Lamellar opercula have been again sub-divided, according to the position of the nucleus or point from which it starts growing, into the following groups :—

Concentric or *annular* (Fig. 3 *a b*), when the nucleus is central or nearly so, and growth takes place all round to a nearly equal extent. The principal genera having concentric opercula are : *Paludina*, *Ampullaria* and a portion of *Murex*. The operculum of *Vermetus* is perfectly circular (Fig. 3 *a*).

Unguiculate, or claw-shaped, when the nucleus is apical (Fig. 3 *e*), among others, *Strombus*, *Fusus*, *Fasciolaria Turbinella*, *Conus*, *Pleurotoma* and *Terebra* have unguiculate opercula.

Or the nucleus may be marginal growth taking place only on one side. If the nucleus is next the *columella* (the pillar or axis round which the shell is coiled) it is said to be *internal* (Fig. 3 *c*), as in *Cassis* and *Pyramidella*, and external if next the lip (Fig. 3 *d*) as in *Purpura*, *Buccinum*, *Phorus*, etc.

When the operculum grows only on one side and revolves as it grows, a spiral is the result. It may be *multispiral*, or many-whorled (Fig. 3 *h*), as in *Potamides*, *Cerithidea*, *Turritella Trochus*, *Delphinula*, *Cyclophorus*, etc. Or it may be *Paucispiral* (Fig. 3 *g*) as in *Cerithium*, *Litorina*, *Nerita*, *Turbo*, and *Cyclostoma*.

In *Trochus* the number of *whorls* or turns in the operculum is sometimes as great as twenty; the number, of course, does not depend on the number of whorls in the shell, but on the size and curvature of the aperture.

It will be seen that no hard and fast line can be drawn between these groups, each one merging into all the others ; even the primary division into spiral and lamellar cannot always be adhered to, the connecting link being that form known as *sub-spiral* (Fig. 3 *f*), as in *Melania*, *Planaxis*, and *Phasianella*.

In spiral opercula, upon the addition of fresh matter to the end of the last whorl, the operculum in order to occupy the same position in the mouth of the shell must make a slight turn backwards on its centre, the nucleus, which is therefore always included in the scar. But in lamellar opercula (which have no rotatory motion) the nucleus and scar are often far removed from each other.

Spiral opercula are always *sinistral* (or coiled to the left) in dextral shells, and dextral in sinistral ones.

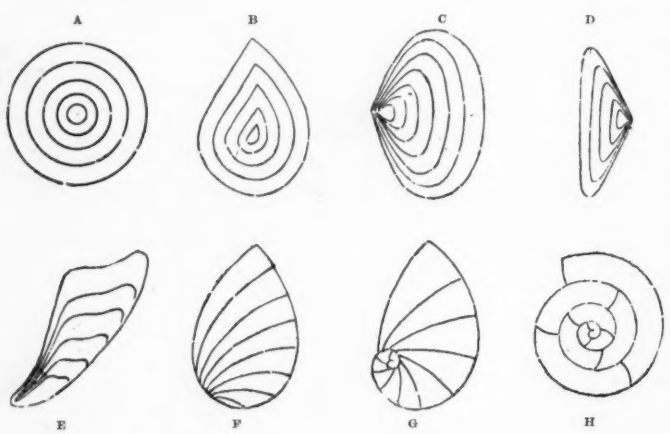


Fig. 3 —Diagram of various opercula.

The operculum of *Siliquaria* (Fig. 4 *a*) is at first sight somewhat peculiar; it is, however, to be regarded as a lamellar operculum, which, instead of growing by the addition of fresh matter to its margin only, increases by the addition of a fresh layer to its inner face. Each successive layer being larger than its predecessor, the characteristic conical form is produced. In like manner the operculum of *Torrinia* (Fig. 4 *b*) is to be regarded as being a multispiral, which increases in thickness as well as in circumference. In both cases the nucleus is, of course, at the extreme point of the cone.

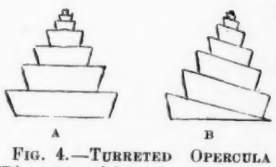


Fig. 4.—Turreted Opercula (Diagrammatic).

Sometimes the operculum is *articulated* with the shell by means of projecting processes or teeth, as in *Nerita* (Fig. 5) and *Jeffreysia*.

Opercula are often spoken of as being either horny or shelly. Now this distinction is somewhat misleading. As a matter of fact, *all* opercula are horny, though in several genera, such as *Turbo* and *Natica*, they are coated with a layer of shell on the outside, and in *Nerita* it is also lined with shelly enamel on the inside. This external layer of shell is often very remarkable. In the section *Senectus* of *Turbo* it is smooth and solid, in some large specimens attaining a thickness of three-quarters of an inch, thus offering a formidable barrier to its foes. In the section *Sarmaticus* of the same genus the exterior has a peculiar *botryoidal** structure, reminding one of the tufaceous deposit of some "petrifying springs"; while some forms of *Natica* and *Turbo* have the operculum ribbed on the exterior.

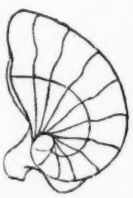


Fig. 5.—Operculum of *Nerita* (Diagrammatic.)

Too much importance must not be attached to the operculum as an aid to classification, being often present in some species and absent in others of the same genus (Adanson divided the Gastropoda into two groups, according to its presence or absence, calling the operculated forms *Sub-Pelecypoda*, under the impression that they formed the connection between the inoperculated forms and the bivalves!). In *Ampullaria* it is, indifferently, horny or shelly.

There has always been considerable diversity of opinion as to the homological relations of the operculum; several naturalists have considered it to represent one valve of the *Conchifera*, while according to Dr. Loven it represents the *byssus* (or beard) of the bivalves. The latter is the opinion of most conchologists at the present day.

The operculum makes its first appearance comparatively late in the embryonic life of the Gastropod, being formed at a much later period than the shell, which it resembles in its mode of formation. A gland appears on the upper side of the meta-podium, being formed by an invagination of a portion of its surface, within which the nucleus of the operculum is formed; the gland soon flattens out again, thus bringing the operculum to the surface. With the exception of the *Pulmonifera*, all Gastropods possess an operculum during embryonic life, even if it is absent in the adult.

* From the Greek, *botrys*, "a bunch of grapes," and *eidos*, "like."

XUM

THE NORFOLK BROADS.

Printed by the Collotype Process from a photograph by Mr. Payne Jennings.

Direct Photo Eng. Co., Limited 9, Barnsbury Park, N.

In the air-breathing gastropods (*Neurobranchiata*), there is often some arrangement to enable the animal to breathe when the aperture is closed by the operculum; thus, in one section of *Cyclophorus* (*Rhiostoma*), there is a small tube from the top of the body-whorl. In *Cataulus* (section of *Pupina*), a channel in the base of the body-whorl admits the necessary air (Fig. 6, *b*.) In *Helicina* there is a notch either in the operculum or at the side of the mouth, while *Pupinella* (Fig. 6, *a*) has a small slit in the *peristome* (or margin of the aperture).

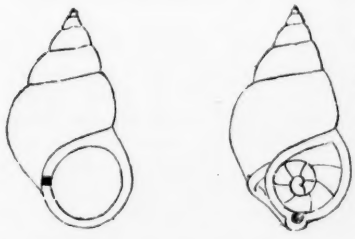


FIG. 6.—DIAGRAM, shewing arrangement for admitting air when the mouth is closed by the operculum.

The possession of two or more opercula is a not uncommon monstrosity in *Buccinum*, *Purpura*, etc. When this is the case, the opercula are either small and distinct, or, being of the normal size and attached to each other, are very much crumpled up, in order that they may fit inside the mouth.

If by some accident the operculum is lost, it does not seem to be reproduced, but the portion of the integument opposite the mouth becoming hard and horny makes up for the deficiency.

The shell of *Hipponyx* is supported and attached to the rock by a peculiar shelly base, which has been regarded as a modified operculum by many naturalists. Its homological relations are, however, quite different. In the first place, it is situated *under* the foot, while the true operculum is always formed on the upper surface; secondly, it is shelly throughout; thirdly, the scar is horse-shoe shaped (whence its name), being thus unlike the scar on the operculum, which is always more or less ovate; and last, but not least, *Hipponyx*, during its free-swimming larval stage, has an operculum which is lost before the animal settles down for life. Now, this structure is really formed by the calcification of the (so-called) sole of the foot, the scar being caused by the termination of the *pedal* branch of the *M. columellaris*, which, as I said before, is always inserted in the foot.

Clausilia when nearly adult secretes an elastic calcareous filament, which is really one of the folds on the columella modified, one of whose ends is attached to the columella, about one whorl from the mouth, the other being free. When the animal is completing the aperture of its shell it secretes a calcareous lamina on the free end of this filament, which, being of the same contour as the inside of the aperture, acts as an operculum. This structure, which was first noticed by D'Aubenton in 1743, is known as the *clausulum*, for which reason Draparnaud gave the genus the name it now bears.

The animal on coming out of its shell pushes the clausulum against the columella, and on retiring the clausulum closes again in consequence of the elasticity of the filament by which it is attached to the shell.

During the winter many of the *Pulmonifera* hybernate, retiring far into their shells and closing the aperture with a layer of hardened mucus, usually strengthened with earthy salts, particularly calcic carbonate and phosphate. Draparnaud gave this false operculum the name of *epiphragm*. Unlike the true operculum, the epiphragm is discarded as soon as the animal awakens, and a new one formed at the commencement of the next season of torpidity. The epiphragm is in warm climates also formed during the summer by such species as *æstivate*, or indulge in summer sleep, but it is not usually so solid as that formed in the winter.

The epiphragm first starts as a ring round the interior of the aperture, it is gradually enlarged towards the centre, the opening being thus contracted until the mouth is quite closed. The epiphragm, however, always remains porous towards the centre in order to admit the air necessary for respiration (respiration and circulation *not* being entirely suspended during torpidity, as is sometimes asserted).

During severe weather accessory epiphragms are sometimes formed behind the principal one; Gaspard has counted as many as six such accessory ones.

THE COLLOTYPE PROCESS AND PHOTO-ENGRAVING.

BY A. C. RANYARD.

THE picture on the opposite page, and all the recent photograph-like looking pictures published during the last year in KNOWLEDGE, have been printed by the Collotype process. Our readers will probably be glad to have some explanation of how the reproduction is accomplished, and it will be a convenient opportunity for explaining to contributors and friends who may think of sending photographs, the sort of negative or paper print which is most suitable for reproduction. All paper prints have to be photographed, and the photographic copies or negatives thus obtained have to be copied again by contact printing on to the thick glass plate from which the printing is done in a printing press. This necessitates at least two copyings, in each of which detail is lost, for there is no such thing as producing a copy which shows all the gradations of tint in the original.

Thus it will be evident that the best work can only be produced when the original negative is available, and even then the glass on which the negative is taken may render it unsuitable. Much of the glass used by photographers is very far from being perfectly flat. The small amount of curvature that generally exists makes but little difference when the negative is used for printing upon paper, for the paper can be pressed into contact with the surface of the glass by the cloth backing of the printing frame. But for the Collotype process the negative has to be printed on to another glass plate, and unless there is absolute contact the print produced will not be sharp. It is therefore necessary to make use of negatives on perfectly flat glass, or on flexible films. The latter are preferred by the Collotype printer, because they can be used for printing from either side, and thus the right and left hand sides of the resulting picture can be reversed. It will be evident, from the explanation already given, that if the film were not reversed, before it is used for printing on to the printing block, the paper which comes off the printing block would, like the negative, be reversed right and left, so that words would read backwards. As this is frequently of consequence, photo-engravers and automatic process printers either take their photographs after reflection in a mirror, or the photograph is taken on a film or on a stripping plate. A stripping plate is a plate on which the film is only attached to the glass round the edges of the plate. When a photograph has been developed and dried on such a plate, a knife can be run along the edges, and the film torn off. The film is then reversed and used for printing on the printing block, which is a thick sheet of plate glass

coated with a film of bichromated albumen, covered by another coat or film of chromated gelatine, which is changed by the action of light so as to alter the amount of water it will take up. The plate after being carefully dried in a drying-box, in which it is kept accurately level for an hour or an hour and a half, at a temperature of 50 cent., is exposed under the negative, and washed, when a peculiar grain or net-like formation becomes apparent. It arises in consequence of the uneven distribution of the soluble layer in comparison with the substratum. The effectiveness and artistic appearance of the print produced greatly depends on the size of this grain, which should not be too large. In the accompanying plate, and in the picture of the sun-spot, it needs a hand magnifier to show it.

The printing block when placed upon the printing-press is damped with glycerine and water. The parts of the film which have not been acted upon by light absorb water more freely than the parts which have been so acted upon, and the absorption seems to be in inverse ratio to the intensity of the light action, so that when a roller charged with a fatty ink is passed over the plate, different proportions of ink are taken up by the dry and wet gelatine in very exact proportion to the action of the light. The damp gelatine refuses to take the ink, while the dry takes it and prints black upon the paper. Thus a picture is produced showing the most delicate gradations of light and shade.

The sort of negatives which enable the Collotype printer to produce the best results are those which are quite free from fog or yellow stain, and are not too dense. For most subjects a slightly under exposed rather than an over exposed negative is preferable. A printing block usually only produces 600 to 800 printed copies, and the process of printing is necessarily a slow one, as the gelatine surface of the printing block needs to be moistened with the glycerine and water solution after every batch of 80 or 100 copies; and in some conditions of the weather, and with some gelatine, the press needs to be stopped, and the surface of the block moistened, at much more frequent intervals.

The photographs of the tornado in the September number, and most of the lightning photographs in the June, July, and August numbers, were from paper prints. Much better results would no doubt have been produced if the original negatives could have been obtained. But where this is not possible, a better result than from a paper copy or silver print can be produced from a contact copy made from the original negative on a flexible film, such as the Eastman Dry Plate Company supply both in rolls and in separate cut sheets.

Most of the other illustrations in KNOWLEDGE are produced by photo-zinco engraving, or etchings on zinc blocks from photographs of pen-and-ink sketches, generally supplied by the authors of the papers. The best sketches are made on Bristol board, or other white cardboard. They should be on a larger scale than the block which it is intended to produce, so that the drawing will bear reducing to about two-thirds of its original size; and the shading should be carefully done with sharp ink lines which are equally black throughout—different depths of shading being produced by the thickness and closeness of the lines—and not, as artists are frequently inclined to do, by blotting their fainter shading before it is dry. No tints should be used; and all lines, whether thick or thin, should be equally black. Some artists use ebony stain instead of ink; but any good black ink is suitable.

SOME PROPERTIES OF NUMBERS.

By ROBERT W. D. CHRISTIE, Head Master, Wavertree Park College, Liverpool (Member of the London Mathematical Society, &c., &c.).

THERE are four methods at least of obtaining the figures of a circulating decimal:—(1) By the ordinary method of division; (2) by multiplying continuously by X, beginning with the last figure as explained in last month's issue; (3) by the new method of division; (4) by division by **X**. I proceed briefly to give an easy example of each mode:—

(1) $1/7 = 1\cdot000000 \div 7 = \dot{1}42857\dot{}$.

(2) $1/13 = \cdot\dot{0}7692\dot{3}$.

Here we put down the last figure 3 since N in $1/N$ ends in 3.

Our multiplier is 4 since $(3N+1)/10 = 4$.

We then say $4 \times 3 = 12$, put down 2, carry one, and so on as previously explained.

(3) Find the period of 1/271.

Our multiplier for numbers ending in unity is $(N-1)/10 = 27$, as explained in November issue.

We have then:—

9999·9
243
975·6
162
81·3
81
·0·0

Thus $1/271 = \cdot\dot{0}0369\dot{}$.

(4) Find the figures of 1/79.

Put down the last figure which is 1. Get the divisor which is $(N+1)/10 = 8$. We proceed as follows:—

8)1012658227848
0126582278481

Say 8 into $1 = 0$ carry 1. Now put this 0 beside the 1, and say 8 into ten (10) = 1 carry 2; put down the 1 beside the 0 (101) and say 8 into $21 = 2$ carry five; put down the 2 beside the 1 (1012), and say 8 into $52 = 6$ carry 4; put down the 6 beside the 2 and say 8 into 46, &c.

The process is perfectly mechanical, and may with practice be done very speedily. The process stops when the figures repeat.

By means of (2) we may considerably shorten the work. An example will best illustrate the method. We have $1/19 = \cdot\dot{0}52631578947368421\dot{}$.

The multiplier, viz. 2, is so easy in this particular case that no advantage can accrue in point of speed, but in most cases there is a distinct gain.

Suppose we have proceeded with the multiplication, beginning with the last figure, as far as the *sixth* figure, say, 368421. We may now get six others (and often more) very speedily by multiplying these figures by 7 (since 7 is the residue of $2^6/19$). We can now write down the last *twelve* figures of the period, viz. 578947368421. Proceeding, we may now multiply these twelve figures (so far as may be necessary to complete the period) by 11, since the residue of $2^{12}/19 = 11$. The process is perfectly general, and may be applied to any proper fraction $1/N$ where N ends in 1, 3, 7 or 9.

By this process we may obtain the result of, say, $1/487^2$ with an easy multiplier of one or two figures.

In this connection a very useful Formula for Circulating Decimals is $(P-1)(H_1+1) = H_2+1$ where P signifies the Prime whose period is sought, H_1 the figures of the first half of the period, and H_2 the figures of the second

half. Thus, *e.g.*, $1/73 = \cdot\dot{0}1369863\dot{}$. Here we have $(73-1)(137) = (9863+1)$.

Thus given either half of a circulating decimal we can readily obtain the other half. There is always a complementary fraction; thus, also, $(137-1)(72+1) = 9927+1$, therefore $1/137 = \cdot 00729927\dot{}$.

From what has already been given various other interesting but more elaborate formulæ may be obtained.

There is a close connection between the Periodicity and the Divisibility of Numbers.

The sum of the digits of any integer divided by 3 or 9 will leave the same remainder as the integer divided by 3 or 9. Note that the period of 1/3 and 1/9 is 1.

Again, a number is divisible by 11 when the difference between the sum of the digits in the odd places and the sum of the digits in the even places is divisible by 11.

Note that the period of 1/11 is 2.

A similar law prevails if we take every *third* figure. If we take, *e.g.*, the number 765234, we shall find that it is divisible by 37, since $4+5 = 3+6 = 2+7$, and any number which fulfils the same condition is of Form $37M$. Also any proper fraction having a period of $3n$ figures, then the figures of the period are exactly divisible by 37.

Note that the period of 1/37 is 3.

Again, if we take every fourth figure of an integer and find the sums equal, we shall find the integer divisible by 101, since the period of 1/101 is 4. When the period of the proper fraction $1/N$ consists of $4n$ figures, then it is of Form $101M$, *e.g.*, $1/73 = \cdot\dot{0}1369863\dot{} = 101M$.

Similarly if we take every *fifth* figure the number is divisible by 41, and similar laws prevail.

Again, if the periodicity consist of $6n$ figures, then the figures of the period are exactly divisible by 7 and by 13; and if the sums of every sixth figure of a number be equal, then the number is divisible by 7 or 13.

We may pursue this to any extent, and obtain tests of divisibility for any number whatever ending in 1, 3, 7 or 9.

Again, we may note that if the period of a recurring fraction be an even one, then the sum of the figures of which the period consists will be $9p/2$; and if the period be an odd one, the sum is $9(p-1)/2$. Also, it will be found that the sum of the *remainders* is $Pp/2$ if *even*; and $P(p-1)2$ if *odd*. Here P means of course the prime, and p the period of $1/P$.

Thus, *e.g.*, $1/7 = .\dot{1}42857\dot{}$ and $1+4+2+8+5+7 = 9 \times 6/2 = 27$.

Again, $1/79 = .\dot{0}126582278481\dot{} = 9(13-1)/2 = 54$.

We find the *remainders* to be 10, 21, 52, 46, 65, 18, 22, 62, 67, 38, 64, 8, 1, whose sum is $79(13-1)/2 = 474$.

This question of *remainders*, however, is so interesting and opens up so many novel points, that I propose to treat it more at length in a future paper.

WALKS IN THE SYDNEY BOTANIC GARDENS.

By C. PARKINSON, F.G.S.

WHEN the late Sir John Hooker visited Sydney some forty years ago in the *Erebus*, he found the already famous Botanic Gardens in a state of transition. The original enclosure was rectangular and stiff in arrangement; it was crowded with tropical and sub-tropical rarities from all quarters of the globe, but lacked the beauty of landscape art—which we in England owe to Paxton. It has been the persistent aim of Mr. Charles Moore, F.L.S., the director during this lengthy period, to unite the scientific and the picturesque: and how successful his efforts have been the result amply proves. Situated on sloping ground which skirts one of the quiet bays of Port Jackson, no finer site could have been chosen throughout the whole world. But to avail himself of the full capabilities of the natural configuration of the land, it was necessary to reclaim about fifty acres from the sea, rounding off the sweep of the bay and securing valuable rock features for adaptation in the future garden. Sir John remarked that a rocky wilderness was to be incorporated with the Botanic Gardens, and added that it appeared to him a useless labour owing to the sterile nature of the ground. In spite of difficulties the work was patiently carried forward. A sea wall was built to resist the incursion of the tide. The space intervening was gradually filled in with rich soil, and the water-worn rocks of the old coast-line now remain exposed a hundred yards inland. The whole of the undulating ground is in the highest state of cultivation, and the additional space comprises the most attractive portion of the noted Sydney Gardens, which fringe the harbour within five minutes' walk of the centre of a busy city.

For a stranger the best approach to the gardens is to follow the circular quay, entering by the gate where the sea-wall protects the road and the private grounds of Government House. From this side the bewildering floral beauties are successively unfolded, and a more comprehensive view is obtained than from the Domain side. It was in February—midsummer in these parts—that I found myself at this spot with a temperature of 86° Fah. in the shade. A glorious oleander grew to the right of the gate, standing nearly 20 feet in height and covered with double blossoms of deep rose colour—a foretaste of what might be expected within.

Away as far as the eye could reach close-cut lawns extended, broken by brilliant flower-beds, with noble trees dotted here and there, and winding paths. The undulating character of the ground is such that each turn develops fresh beauties, and the refreshing ripple of the waves is rarely absent. Men-of-war ride at anchor on the blue waters of the harbour, which is capacious enough to shelter all the navies of the world in perfect safety. There are no restrictions with regard to walking on the grass; in fact, innumerable seats offer an inviting shade beneath the spreading foliage. Hundreds of people may be seen at any time of the day enjoying the bright surroundings. Smoking only is prohibited, and the whole city seems at home in its pleasure grounds; wilful damage or stealing of plants is a crime unheard of. As the feet come in contact with the turf unaccustomed visitors are conscious of a peculiar elasticity beneath the tread, quite different from English lawns. Although delightfully green (there was abundant rain in this month), and trimly kept, the herbage is coarse in texture. It is the buffalo grass (*Stenotaphrum glabrum*), a succulent species carefully selected in all antipodean gardens for rapidity of growth and its power of resisting the effects of drought. The springy sensation in walking across these lawns once experienced can never be forgotten. Seated underneath a fig-like banyan tree with rooting branches hanging downwards, I can better realise the happy combination of sylvan scenery with the animated harbour surroundings; the water is alive with smart centre board yachts, sailing to and fro in reckless fashion, local steamers swarming with passengers; and ocean liners are buoyed in the channel ready for departure. Nearer to the beach strange incidents are apt to occur. It may be a retriever disporting in the sea is seized by a rapacious shark. No one ventures to bathe in unprotected water; certain spaces are secured by palisades, for the coast is literally infested with hungry monsters. Leaning

over the sea wall you may gaze at colonies of living oysters attached to rock beneath the clear waters; strange Pacific fishes lurk in the weeds, and delicate algæ flourish at the bottom of the sea. It is indeed a charming haunt, and one might linger content for an indefinite time where all is so novel and fresh.

Penetrating a little farther into the gardens the dazzling brilliancy of the single scarlet hibiscus rivets the attention. These shrubs have dark glossy leaves and large flowers, which, alas! fade with the setting sun. It is familiarly known as the "shoe" plant, from the fact that a substitute for "blacking" (or ink) can be extracted from it. Yellow, rose, and several double red species of hibiscus are freely distributed, and this representative of the Mallow order has evidently found a congenial home.

A favourite seat of mine was beneath the shade of a great bamboo, by the side of a miniature lake. Curious Japanese waterfowl swam in and out among the yellow water lilies (*Nymphæa lutea*). A sociable rat (*non-marsupial*) repeatedly stole the duck's food, undeterred by my presence or the protests of the rightful owners. The unsupported stems of the bamboos sway imperceptibly in the light breeze, creaking against each other with uncanny sounds, while the leaves shiver and rustle in a manner peculiar to themselves. There are various kinds with yellow, green, or variegated stems. The yellow water-lily has flowers equal in size to our English white species, with petals of pale yet rich tint. Side by side the Australian blue water-lily (*N. gigantea*) thrives with a scarlet-flowered *N. Deroniensis*. A curious yellow hybrid had developed by pure accident about this time, and had been placed in a separate pond. Without apparent cause the leaves died down, and were eaten away; a new slug attacked the under surface of the leaves, I believe the larvæ of which had been introduced with the roots of the water-lily. The life-history of this parasite, which only touched the one species, has not yet been worked out.

Opposite to my bamboo clump there was an islet luxuriantly covered with the dracæna from New Zealand, native cabbage-palms, and noble tree-ferns (*Dicksonia* and *Alsophila*) from the Blue Mountains. Vivacious and bright-coloured birds are by no means the least attractive feature in the gardens. The azure wren hops unconcernedly around, with tail cocked in the air. The hen is brown, with an ash-coloured breast; the male has turquoise blue on the head and back, in marked contrast to the jet-black throat and brown tail. A little farther on a salmon-breasted honey-eater, with prolonged and curved bill—specially adapted for the extraction of nectar—deftly clings to a branch of cinchona, busily searching the corolla tubes. Black and white fly-catchers hover in the branches of the eucalypti, and a small warbler-like bird, known as the "silver eye"—from the white circle round the eyes—twitters merrily in the bushes.

To speak of the eucalyptus is to use a wide generic term; there are upwards of sixty well-marked species indigenous to Australia. The blue gum (*E. globulus*), introduced freely in the South of Europe, and occasionally met with in England, I hardly saw in New South Wales; it is a native of Tasmania. Around Sydney I think the red gum (*E. rostrata*) and several other species prevail. But for ornamental purposes the scarlet-flowered gum (*E. ficifolia*) of West Australia stands pre-eminent. It is a shapely tree with regular and abundant foliage; the wood is sound, the bark does not hang in shreds, nor the branches die off prematurely; at certain seasons it is a blaze of crimson blossom. I do not think so much can be said of any other eucalypt.

Wandering from tree to tree, I was struck with the sense of restlessness on all sides; there is never absolute stillness in the air, however motionless or oppressive the atmosphere; the cicadæ are seldom quiescent, and their name is legion. It is somewhat irritating to the nerves of a new comer to hear the constant "chirp, chirp" from an invisible world of creatures. You may search in vain; until their ventriloquising habits are mastered, not a single one will be found. I hunted the branches and rocky crevices for half an hour before discovering a brown, cricket-shaped insect, which was hidden all the time under my very nose. This sound-laden air is a characteristic of all Australian gardens, but the ears of residents are deadened to the accustomed noise.

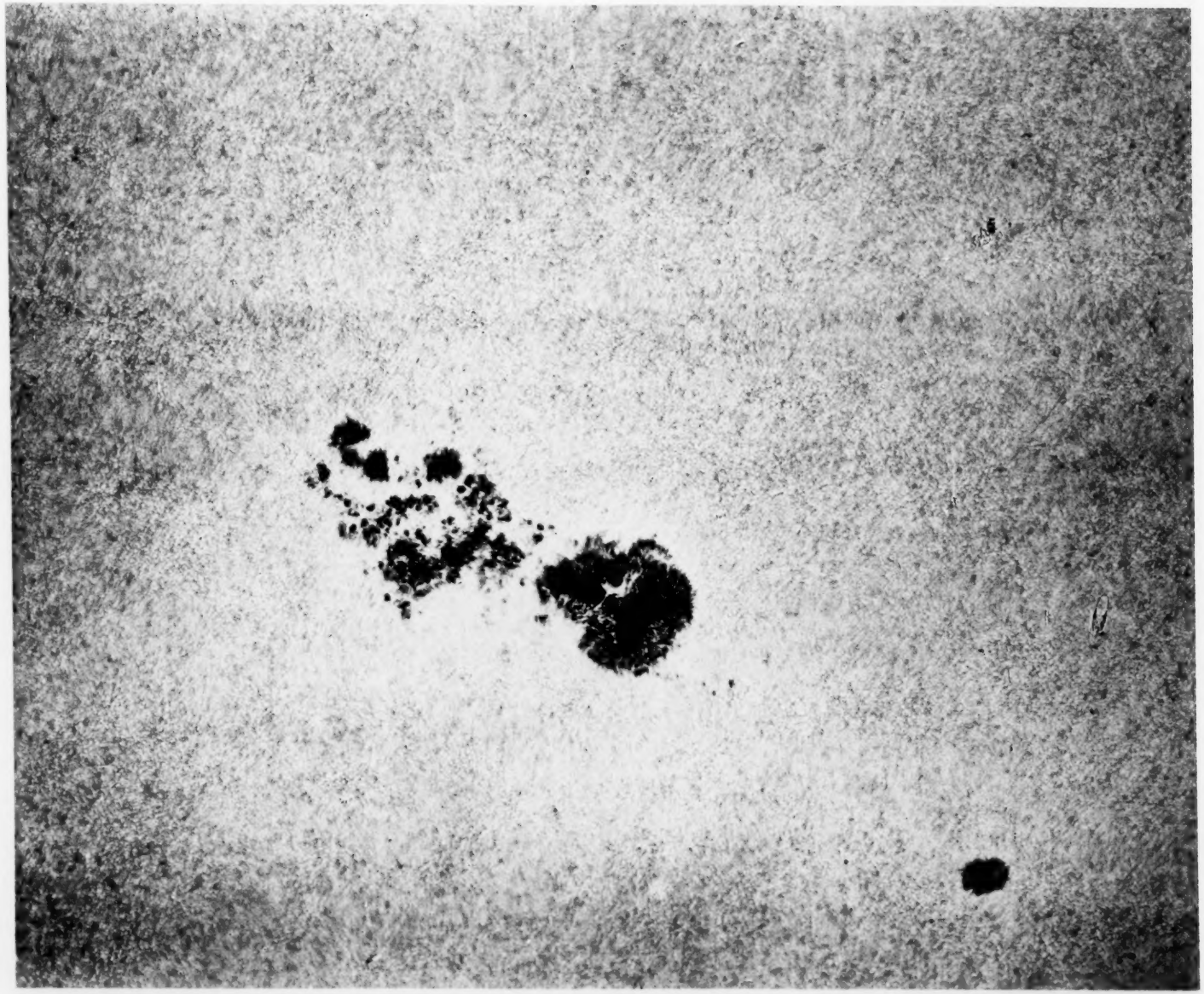
Great butterflies lazily flit from flower to flower. I noticed one, bigger than the swallow-tail, which could not resist the seductions of the border plants in one part of the garden, consisting of masses of the rosy-pink lily (*Amaryllis belladonna*) just then in the height of perfection; it was fascinated apparently by the one colour. The profusion of liliaceæ and amaryllideæ was most confusing. The flower spike of one species—*Doryanthes excelsa*—from Queensland, stood many feet high, and when cut, was as much as a man could carry. Agaves (aloes) in great variety grow freely in the open air, and some curious cactiform euphorbiaceæ were of striking appearance, notably *Euphorbia splendens*, with scarlet involucres, well known in English hot-houses, but in Sydney flourishing in the flower borders. Poinsettia trees grew fifteen feet high; at the first approach of autumn the terminal bracts assume a crimson tinge.

It is easy to trace the old water-line in the middle of the gardens; the rocks, furrowed by the action of the waves, still indicate the natural shore. Rugged Moreton Bay figs (*Ficus macrophylla*) spread over the ledges, affording a rare opportunity for the study of their remarkable root growth. The underground branches suffer all manner of contortions, eating a way through the most impossible places. Roots cling to the bare surface where no soil is. It forms a valuable shade in the principal parks and public gardens of Sydney. In the Domain cattle diligently chew the fallen leaves and fruit with evident satisfaction, preferring the sweet flavour to that of coarse grasses. Giving ample shelter from a vertical sun, there is one objection to the cultivation of the Moreton Bay fig in the vicinity of houses: the roots penetrate everywhere, breaking drains and forcing a passage through the strongest walls. Sometimes the branches develop falling shoots, which take root after the manner of a near relation, the banyan (*Ficus indica*).

(To be continued.)

PHOTOGRAPH OF THE SUN'S SURFACE.

THE accompanying picture has been produced, without any touching, from a remarkably beautiful photograph of a sun-spot taken by Dr. Janssen, who has succeeded better than any other observer in photographing the sun's surface. It was taken with a telescope of only about five inches aperture, on a wet collodion plate. Our enlarged copy shows as much as can be seen under the best conditions with the eye at the telescope; and the negative from which it is made shows still more. On the negative the brilliant bridge which stretches across the great spot is seen to break up into a number of distinct elongated masses, and the whole of the solar surface is seen to be composed of somewhat similar bright masses arranged in places in a curious pattern, as



PHOTOGRAPH OF A REMARKABLE SUN-SPOT, TAKEN BY DR. JANSSEN, SHOWING THE WILLOW-LEAVED STRUCTURE OF THE PENUMBRA OF THE SPOTS AND THE GRANULATED STRUCTURE OF THE WHOLE OF THE PHOTOSPHERE.

Direct Photo Eng. Co., Limited, 9, Barnsbury Park, N.

if combed out, while in others they appear hazy and indistinct. This local "smudginess" is no photographic defect, such as might possibly be produced by currents of heated air in the telescope tube, or clouds of vapour, as others have suggested, rising from the damp collodion surface when struck by the flash of brilliant sunlight during its exposure, for Janssen has found that pictures taken in immediate succession show the same "smudges" on the same parts of the solar surface, which would, of course, not be the case if they were the result of accidental currents or vapour in the tube of the telescope. But the areas of haziness and the arrangement of the granules of the photosphere change very rapidly as compared with the general rate of changes in sun-spots. Two photographs of the same region taken by Janssen with an interval of fifty minutes are given in the *Annuaire* of the Bureau des Longitudes for 1879. They show the great amount of change which takes place in small details, while the main features of the groups of spots, and spots, remain constant. When the immense scale of the objects we are looking at is remembered, the rapidity of the changes taking place becomes evident. The whole sun is covered with what Janssen describes as a reticulation of hazy areas, which are continually shifting their place. We are looking down on the photosphere in which the "rice grain" or "willow leaf" structure is seen here and there so distinctly, through the whole depth of the corona and through the chromosphere. Though we know nothing at present as to the rate of the changes taking place in the corona, we know that the forms of the prominences in the chromosphere change very rapidly as compared with the forms of the spots, so rapidly that the rapidity of the changes in the form of the hazy areas does not preclude us from assuming that there may be a connection between the disturbances in the chromosphere and the indistinctness of the details of the photosphere seen through them. Such a hazy appearance might be produced by a dust veil or smoky precipitate in the prominence region, or it might be produced by refraction in a disturbed gaseous atmosphere causing indistinctness of vision. What we know of the density of the gas in the prominences from the breadth of the lines in their spectrum would lead us to suppose that the gaseous pressure is very small compared with our atmospheric pressure; but we have no evidence to show that the density of the gas is sufficient to deflect a ray of light through a few seconds of arc, a deflection which would be quite sufficient to account for the indistinctness of vision observed. We know that the disturbances in the prominence region are very great, and it seems probable that the differences of temperature between the uprushing gas and the surrounding medium are very considerable.

On the other hand there are some considerations which seem to render it improbable that the density of the gas in the prominence region can be sufficient to give any observable deflection of a light ray. With gravity more than twenty-seven times as great as terrestrial gravity, and the whole density of the sun only a little more than a quarter of the density of the earth, it seems improbable that the gas of the prominence region can exist as an atmosphere, with each layer resting on the layer below it. And if the gas is not in the condition of an atmosphere, but the molecules are moving in immensely long free paths, it is difficult to conceive of an aggregation of molecules sufficiently dense to deflect light rays. We are not, however, forced to make this supposition, for the coronal region is known to be full of dust, which causes the strong polarization of the coronal light, and it seems natural to suppose that there would be a much greater precipitation of solid particles in the denser uprushing gas of the prominence region. Such a smoky precipitate would, no doubt, cause a veiling of objects seen through it.

The much greater brightness of the area surrounding the spots is well shown in the plate, as well as the greater brightness of the inner edge of the penumbra, a phenomenon which is very marked in other photographs besides those of Dr. Janssen.—A. C. R.

Notices of Books.

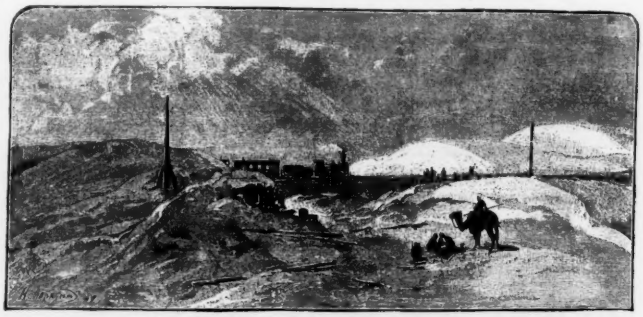
Chemical and Physical Studies in the Metamorphism of Rocks. By A. IRVING, D.Sc., B.A., F.G.S. (Longmans, 1889.) Desirable as it is that universities should recognise original work in conferring degrees, there are few more dreary classes of reading than ordinary graduation theses, in which young men generally enunciate a series of platitudes in a dead language with all the sublime airs of pioneers of the human mind. Very different in character from such essays is Dr. Irving's slim volume of studies, though it is based professedly on his graduation thesis. Few Englishmen have such great experience in both the laboratory and the field as he evinces in this eminently suggestive little work. We are prepared, and we think that many geologists will be prepared, to adopt very many, if not most, of Dr. Irving's conclusions. The main position defended may perhaps be stated as being that the Archæan rocks represent the primordial crust of the earth, and owe their crystalline character, not to any process of metamorphism, "regional" or otherwise, but to deposition from a dense primitive atmosphere under conditions of temperature at which ocean-basins would have been impossible. The wealth of chemical argument, necessarily highly technical, with which this position is supported, seems somewhat undigested, or, at all events, has not been so arrayed as to make the essay very "readable"; but this may perhaps be partly explained by the brevity of the work and its construction by the annotation of a yet shorter paper. The author does a service to geologists in general by directing their attention to Dr. Percy's work on Fuel and other portions of his Metallurgy, and in insisting on the inconclusive character of the argument as to the existence of organic life in Archæan times from the presence of graphite. We are also fully at one with him in advocating precision in geological nomenclature, though it may be doubted whether his own additions thereto tend in this direction; but his reasoning seems to us to gain nothing by his frequent but strained analogies between the classification of rocks and that of plants and between the behaviour of glacier-ice and that of igneous rocks. It would perhaps be just as well, in the light of recent exposures of the "diagrammatic section" in the Scottish Highlands, to directly impugn the accuracy of the section across Errigal, given by Professor Green (*Physical Geology*, p. 401), rather than to attempt to explain it; but the "animus" with which Dr. Irving occasionally allows himself to write is to be regretted. It might well startle anyone to be advised "to rise above the text-book level of looking at the facts"; and when Dr. Irving asks "the scientific geologist" to free "himself from all empirical theories—all fanciful ideas—of 'regional metamorphism,'" he should not be surprised if some "old stagers" exclaim "Physician, heal thyself," or—no doubt, from ignorance—object, with Hutton, to "judge of the great operations of the mineral kingdom from having kindled a fire, and looked into the bottom of a little crucible."

Chambers's Encyclopædia. (Vol. IV.—Dionysius to Friction.) The present instalment of this excellent work

includes articles of special value in science and history, as, *e.g.*, in the former, Electricity, Embryology, Ethnology, and Evolution, and in the latter Druidism, Egypt, and Florence. The article on Druidism is from the competent pen of Professor Rhys, and will therefore further help to clear away the mystical nonsense which has been written on that subject, while the names of the late Dr. Birch and of Mr. Lane Poole guarantee the general accuracy of the article on Egypt. Mr. Thomas's article on Embryology gives an admirably clear survey of a subject of growing significance, including a succinct account of Dr. Weismann's much-debated theory of heredity. And although Mr. Patrick Geddes is included by ultra-Darwinians amongst the neo-Lamarckians, they can take no grave exception to his moderate exposition of the general theory of Evolution. It is always difficult to obey the canons of proportion in books of reference; a distinguished writer, on a relatively unimportant subject, may have space given him to the detriment of articles of permanent importance, and we therefore, perhaps for lack of things more serious with which to find fault, take exception to the allotment of more space to George Eliot than to Erasmus, and of nearly as much space to her as to Mr. Geddes's paper. But in so far as we have been able to glance at the multifarious contents of the volume, we can pronounce it to be as well up to date as its predecessors. Editor and staff and publishers alike have taken Strafford's "thorough" as their motto, and are acting up to it.

Travels in the Atlas and Southern Morocco. By Joseph Thomson, F.R.G.S. (London: Philip & Son, 1889.) The distinguished explorer of Masai Land, sighing for new worlds to conquer, turned his gaze on nearer, but unfamiliar, and, to a large extent, untrodden ground, and started on a journey of discovery through Southern Morocco. Very much to whet a traveller's appetite, and to interest both the geologist and the botanist, lies on the other side of the Great Atlas Range, and Mr. Thomson started with the intention to reach the unexplored Sus valley. But owing to the fanaticism of the Sultan-defying tribes, and the obstructions of the Kaids, his success was only partial, although he scaled heights which no other European had reached, and entered districts hitherto untrodden by the Christian infidel. His narrative lacks the stirring interest which fills every page of *To the Central African Lakes and Back* and *Through Masai Land*; but the journey yielded enough of adventures, amusing and perilous, to make the present book instructive and entertaining. Every reader of it will rise with awakened interest in the future of an Empire so near to our own Mediterranean possessions, and so wrapt in mystery. Two clearly printed maps illustrate the physical character of the districts traversed by Mr. Thomson, and an abundance of woodcuts from his photographs of people and places enrich the text.

Russia in Central Asia in 1889, *and the Anglo-Russian Question.* By the Hon. George N. Curzon, M.P. (London: Longmans, 1889.) Mr. Curzon is a fine type of the small but increasing number of our younger members of Parliament who are worthily equipping themselves for service to their country by gaining personal knowledge of those parts of the world where a watchful jealousy of British interests is demanded. Notably is this the case in Central Asia, where the relations between Great Britain and Russia are being profoundly affected by the Trans-Caspian railway, the primary object of which is military. Mr. Curzon, after overcoming the difficulties of official obstruction, travelled along this newly-constructed line, over shoreless seas of blinding sand, and over the long pile bridge which spans the yellow Oxus, until he reached Samarkand, the present terminus, ultimately travelling as far as Tashkent, the capital of Turkestan. His main object was fuller understanding of the actual state of affairs in Central Asia, but, as his brilliant word-pictures



Railway and Sands.

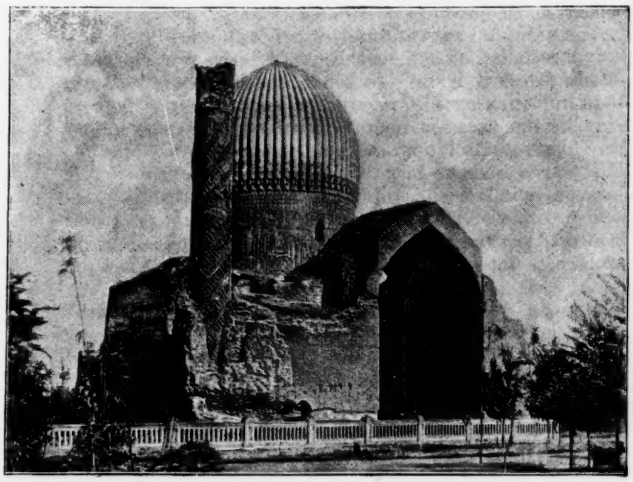
evidence, he kept keen eye upon the manifold attractions of the historic ground which was thus traversed in modern and unromantic fashion. For who can visit Bokhara,

Main Street, Bokhara.

"the most interesting city in the world," and Samarkand, albeit terribly Russianised, threading fearlessly the bazaars of the one, and standing by the coronation stone and sepulchre of Tamerlane (or Timour) in the other; or look on the "shorn and parcell'd" Oxus that, "like the Euphrates and the Ganges, rolls its stately burden down from a hoar antiquity through the legends and annals of the East;" and remain unmoved by the associations kindled by these theatres of ancient and perished civilisations, with their uncurbed tyrannies, their savage splendours?

The judicial attitude which Mr. Curzon strives to maintain throughout is shown in the dedication of his book to Russophobes and Russophiles alike, it being "equally disrespectful to the ignoble terrors of the one and the perverse complacency of the others." And it is most satisfactory to meet a writer who, after examination *in loco* of the relations between two great Empires, and after

personal intercourse with distinguished Russian officials, encourages us to regard the movement of Russia in Central Asia without "Mervousness," since if she "held both Herat and Candahar she would not be much nearer to the conquest of India." But while this is the general conclusion reached, Mr. Curzon, who is not always consistent with himself, shows that the approach of Russia to our Indian frontier must create friction and keep alive the chances of war, the chief incitement to which, he



Tomb of Tamerlane.

remarks, would arise in the outbreak of disorder on the Afghan frontier, giving Russia excuse for intervention. He is fully aware of Russia's methods in diplomacy. "In any event, she will be the aggressor. The limits to British dominions in Central Asia are fixed by natural conditions which we should be insane to ignore or overleap, and which sever us as by oceans from Tatar prairies or Turcoman steppes. The inheritance of these lands, with their historic past, their sordid present, and the mysterious possibilities of their future, has devolved upon a race yet young among nations, endowed with surpassing vitality, and destined to greatness. At least, let us wish her God-speed in the undertaking. A new continent has been usurped and a mighty empire has been won. . . . Russia is required to build a new edifice upon the old foundations, to lift a people from the sloth of centuries, and to teach them the worth of manhood. A few crumbling khanates alone remain as an expiring relic of the past, which, with all its pageantry and its horrors, is shrivelling up like a parchment scroll beneath the action of fire. If Russian brains can only estimate the sense of duty, or even of ulterior profit at a little higher price than ephemeral vainglory, and if Russian hands can desist from flying at the least breath of suspicion to the hilts of their swords, there is no reason why a future of beneficence and even of splendour should not await the Central Asian dominions of the Czar" (pp. 412–414). These sympathetic words from a writer who is alive to Russian methods of "civilization," to her bigotry, and to the commercial and political injury which Russian rivalry is inflicting upon us, notably in Persia, should send our readers to careful study of the book itself, which we are glad to see has already reached a second edition. It is adorned with a couple of maps, for the frontier lines on which Mr. Curzon makes himself responsible, and with an abundance of full-page plates and smaller illustrations, throughout equal in interest to those of which the courtesy of the publishers enables us to present copies. Moreover, the appendices include a bibliography of works relating to the subject-matter of the volume, and a chronology of events in Central Asia from 1880 to 1889.

Matabeleland and the Victoria Falls: a Naturalist's Wanderings in the Interior of South Africa. From the Letters and Journals of the late Frank Oates, F.R.G.S. Edited by C. G. Oates, B.A. (London: Kegan, Paul & Co., 1889.) The re-issue of this book, which was first published in 1881, is opportune. For public attention is being directed to South Africa both as a gold-yielding region, and, so far as the country within the sphere of British influence is concerned, as the scene of Portuguese poachings, which have been summarily stopped. The book is compiled from the fugitive materials left by Mr. Oates, who, unhappily, fell a victim to fever in 1875, as he was leaving the Victoria Falls on his homeward journey. The rich and varied collection of fauna and flora which he had gathered were fortunately preserved by friendly hands, and evidence what loss biology suffered through the premature death—for Mr. Oates was only thirty-four—of so able and enthusiastic a naturalist. The journals have been skilfully woven into an interesting, and in nowise showy, narrative by his brother. They abound in references to the natural history of Matabeleland, and therefore appeal altogether to naturalists, being, a bare allusion or two excepted, devoid of matter valuable to the student of barbaric culture. The Editor has been fortunate in securing the help of specialists of the first rank for the several richly-illustrated appendices, that on ethnology being by the late Professor Rolleston, with certain emendations in the present edition by Mr. Hatchett Jackson; that on entomology by Professor Westwood; while ornithology, herpetology, and botany are treated by Messrs. Bowdler Sharpe, Günther, and Allen Rolfe respectively. The volume is lavishly illustrated, and a series of maps enable the reader to follow Mr. Oates's "spoor" without difficulty.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

IS INFLUENZA COMMUNICABLE?

To the Editor of Knowledge.

Dear Sir,—Opinions differ so much upon this point that it is well worthy of a brief discussion. Some believe that it is infectious, that is to say, communicable from one person to another, but not contagious; that is to say not communicated by actual contact with a person afflicted. Others, again, believe that it is in the air, and, therefore, all persons breathing the same air, or living in the same atmosphere, may be similarly afflicted.

If so, it must have travelled with the wind, but data are wanting to prove this in its dissemination over the continent of Europe. One fact alone is apparent, and that is that it has been propagated from east to west. Showing itself in towns and places distant from one another, and separated by districts that have not been affected, this would seem to show that it is rather infectious than simply conveyed through the air.

It is possible there have been what professional men call "sporadic" cases, by which is meant cases in which

no communication with the afflicted can be traced. But such cases are open to doubt, whereas, on the contrary, the malady has chiefly affected congregations of people, as in public offices, barracks, and crowded dwellings. The experience on the continent has been that when one of a family was afflicted, others of the same household were liable to be so likewise.

Granting that influenza is in the air, and not, as cholera was supposed to be, in the water, there must be something in the air to create distemper. The chemical constituents of the atmosphere are to a certain extent unalterable. There may, however, be an excess of oxygen (ozone), or an excess of carbonic acid, and some have asserted that malaria may be due to the presence of marsh or noxious gases. This is a point open alike to discussion and to scientific enquiry. None but those of limited knowledge or of a presumptuous turn of mind indulge in dogmatic opinions.

But the fact of the air being, as it were, poisoned by deleterious gases is so rare a phenomenon, and is generally so confined in its action, as in wells, coal-mines, &c., that the connection of so wide and far-spreading a malady as influenza with such contamination cannot be readily admitted.

It is an "influence"—an epidemic—that is to say, a disease arising from a general cause, and affecting many people in the same district. Epidemics are generally admitted to be dependent upon certain conditions of the air or food. No one, as far as I am aware, has suggested any connection between the so-called influenza and any deleterious food. Such things do, however, occur. I have known an Egyptian regiment to have been decimated by ergot of rye in the bread.

If in the air, and not consequent upon the presence of noxious gases—the presence of which would in general be detected by the smell or a sense of irritation or suffocation—there must be something in the atmosphere—inorganic or organic—apart from its normal condition, that is capable of carrying disease with it, or of causing a deviation from ordinary health—disturbing the functions or structures of the human frame and giving birth to pain and illness.

Inorganic bodies are rarely known to produce such results. The dust from a volcanic eruption has been known to be carried hundreds of miles without affecting the health of individuals. Animals and even men may be suffocated under very exceptional circumstances, as by the simoon of the desert; but these are such phenomenal occurrences that they may be fairly dismissed from the present discussion.

We come then by the simple process of logic to the last of the possible causes, and that which has attracted most attention in our own times, the creation or transmission of disease by the presence of organic matter, be it mere spores or germs of vegetation, or elementary forms of animal life—so small as to be invisible to the human eye, and indeed in some cases only detected with the greatest difficulty, with the aid of powerful microscopes. The discovery of the presence of such noxious microbes, as they are hence termed, in certain well-known skin and other diseases, induced a further examination, which has led to the including of a great number of maladies in the category of such as are caused by parasites or organic functional disturbers. There still remains the vexed question whether in some cases, as tuberculosis, ulceration, &c., the presence of such organic bodies are not due to the destruction of structure—to the disease in fact—rather than to the living thing found within it. Unquestionably such may sometimes be the case.

But in the instance of an epidemic, it cannot for a moment be argued that previous illness, however much it may predispose a person to be influenced, is in itself the cause of that affliction. The bacillus, the presence of which is supposed to induce cholera, is said to have been detected; but the form, whether that of spores or germs of vegetable life, or of elementary animal life, which gives rise to influenza, has not yet been discovered. There are no grounds for believing that it has any analogy with the epidemic bacillus of cholera, or the bacteria of croupy pneumonia, for it produces a different type of disease. The person afflicted becomes, unknowingly and unwillingly, not only a recipient of a malady, but an active propagator of disease; and it is in this sense, and in this sense only, than influenza can be regarded as an infectious disorder. Happily the disease is not of so virulent a nature as in any way to require isolation, still less removal to asylums for infectious diseases; but the knowledge as to how far it may prove infectious is of importance in regulating intercourse with the afflicted or with affected domiciles; and it also affects the question of the advisability of admitting such cases into ordinary hospitals, or even public institutions. All congregations of persons, at a period when the influenza is present, must be favourable to its more general diffusion.—Yours faithfully,

William Francis Ainsworth, Ph.D.

Hammersmith, 13th Jan., 1890.

P.S.—Since the above was written, Professor Kundrat, of Vienna, has found the lungs and brain of a man who perished from influenza covered with highly-organised microbes, which he calls schizomyceles, and which he believes to be the cause of inflammation in those structures. The discovery has been since confirmed by other medical men. This micro-organism appears also to fix itself upon the conjunctiva of the eye, as well as on the mucous surfaces, by which it is probably conveyed to the internal organs. It is obvious that until antidotes are found to these disease-producing vegetable or animal organisations carried in the air, the treatment of such diseases must remain more or less empirical, and directed rather to the alleviation of symptoms than to the cure or eradication of the complaint.

DO ADDERS PROTECT THEIR YOUNG IN TIME OF DANGER BY SWALLOWING THEM?

To the Editor of Knowledge.

Dear Sir,—Some little discussion has been taking place in the recent numbers of Knowledge, concerning the vexed question among naturalists, as to whether adders do, or do not, swallow their young in time of danger. The following extract taken from William Harrison's *Description of Britaine* will interest your readers; it proves that the myth, if it be a myth, is of very old standing, the date is 1587. "I did see an adder once myself that lay (as I thought) sleeping on a molehill, out of whose mouth came eleven young adders of twelve or thirteen inches in length apiece, which played to and fro in the grass one with another, till one of them espied me. So soon therefore as they saw my face they ran again into the mouth of their dam, whom I killed, and then found each of them shrouded in a distinct cell or pannicle in her belly, much like unto a soft white jelly, which maketh me of the opinion that our adder is the viper indeed." The chief difficulty of taking Harrison's account without a "grain of salt," is the question as to what was the nature of the "distinct cell or pannicle" in which he found them shrouded. However, there seem to be many

authenticated cases on record, by intelligent men, of adders swallowing their young. And Miss Hopley, in her *Curiosities and Wonders of Serpent Life*, devotes an entire chapter to the subject which would well repay perusal by those interested in the subject.

Yours very faithfully,
J. W. WILLIAMS.

To the Editor of KNOWLEDGE.

DEAR SIR,—I have noticed several letters in your valuable journal discussing the question whether vipers swallow their young. Personally, I have had no opportunity of observing vipers, but the following facts observed by my father may be of interest.

My father was born in a mountainous part of New England, where snakes were very abundant, and when he was a boy he came one day quietly upon a bridge over a stream of pure, running water of considerable width, where he saw a large snake, with eight or ten young ones swimming near the middle of the stream. When the snake discovered my father, it was apparently much alarmed for the safety of its young, and, making a little noise, she opened her mouth as wide as she could, and each little snake darted in, and to all appearances were swallowed. The snake then swam swiftly along to the bank of the stream, and disappeared.

My father has also told me that a few years after the foregoing incident he was fishing near an island in one of the Carolina rivers, when he saw a repetition of nearly the same circumstances. This time it was a large water-snake swimming near the shore of the island. These instances seem to prove that water-snakes swallow their young, and, if they do, why should not vipers do the same?

Very truly yours,
R. O. M.

Clifton, January, 1890.

THE FACE OF THE SKY FOR FEBRUARY.

By HERBERT SADLER, F.R.A.S.

SOLAR spots are increasing very slowly indeed in number and magnitude. Conveniently observable minima of Algol occur on the 15th at 10h. 1m. P.M., and on the 18th at 6h. 49m. P.M. Mercury is a morning star throughout the month, but owing to his great southern declination he is not well situated for observation. On the 1st he rises at 7h. 2m. A.M., or 39m. before sunrise, having an apparent diameter of 10″, and a southern declination of 15°. On the 14th he rises at 6h. 5m. A.M., or 1h. 13m. before the sun, with an apparent diameter of 8″, and a southern declination of 18°. On the 28th he rises at 6h. 0m. A.M., or 49m. before the sun, with an apparent diameter of $6\frac{1}{2}$″, and a southern declination of $17\frac{1}{2}$°. He is stationary at noon on the 10th, and attains his greatest western elongation ($26\frac{3}{4}$°) at 10h. P.M. on the 23rd. He is in Capricornus throughout the month. When rising on the 1st he will be about 17′ nearly north of the $5\frac{1}{2}$ magnitude star τ Capricorni; on the 17th when rising the planet will be about 23′ *n.p.* the $5\frac{1}{2}$ magnitude star π Capricorni, which is a close and pretty double star; at the same time the following morning he will be 15′ nearly due north of the $5\frac{1}{2}$ magnitude star ρ Capricorni, another very pretty double star; when rising on the 23rd he will be about 16′ *n.p.* the 6th magnitude star υ Capricorni; and at the same time on the 27th the same distance *p.* a little *s.* of the 4th magnitude star θ Capricorni. The Italian astronomer, Schiaparalli, has recently announced that Mercury revolves on his axis, in the same manner as the moon does round the earth, presenting constantly the same side to the sun; the rotation being accomplished in 88 days. Venus is in superior conjunction with the sun at 11h. A.M. on the 18th. Mars is a morning star throughout the month, rising on the 1st at 1h. 32m. A.M. with an apparent diameter of 8″, and a southern declination of $15\frac{1}{2}$°. On the last day of the month he rises at 52m. after midnight, with an apparent diameter of 10″, and a southern declination of 19°. He is in Libra throughout the month. On the morning of the 2nd he will be about 11′ *s.f.* the $5\frac{1}{2}$ magnitude star ν^1 Libræ. Jupiter is too near the sun to be well observed in February. Saturn is excellently placed for observation, rising on the 1st at 6h. 21m. P.M. with a northern declination of 12° 22′, and an apparent diameter of 18″·0 (the major axis of the ring-system being $45\frac{1}{4}$″ in diameter, and the minor $7\frac{1}{4}$″). On the last day of the month he rises at 4h. 21m. P.M., with a northern declination of 13° 11′, and an apparent diameter of 18″·0. He is in opposition to the sun at 4h. A.M. on the 19th, when he is distant from the earth about $767\frac{3}{4}$ millions of miles. On the evening of the 4th Titan will be about 36″ *n.p.* Saturn, at 7h. P.M. on the 12th he will be 34″ due south, on the evening of the 20th about 45″ *n.p.*, and on the evening of the 28th about 40″ *s.f.* the planet. On the night of the 17th an 8 magnitude star will be about $1\frac{1}{4}$′ south of Saturn. Saturn describes a very short path in Leo throughout the month, but does not approach any conspicuous star. Uranus rises at 11h. 36m. P.M. on the 1st, with a southern declination of 9° 45′, and an apparent diameter of 3″·7, On the 28th he rises at 9h. 48m. P.M., with a southern declination of 9° 36′, and an apparent diameter of 3″·7. He is almost stationary in Virgo throughout February, in a region barren of naked-eye stars. Neptune rises on the 1st at 11h. 22m. A.M., with a northern declination of 18° 54′, and an apparent diameter of $2\frac{1}{3}$″. On the 28th he rises at 9h. 35m. A.M. He is well nigh stationary in Taurus throughout February. No well-marked showers of shooting stars occur during the month. The moon will be full at 1h. 13m. A.M. on the 5th, enters her last quarter at 6h. 51m. P.M. on the 12th, is new at 10h. 28m. A.M. on the 19th, and enters her first quarter at 2h. 6m. P.M. on the 26th. At 1h. 51m. A.M. on the 1st the 6th magnitude star 141 Tauri disappears at an angle of 88° from the vertex, and reappears at 2h. 38m. A.M. at an angle of 355° from the vertex. At 5h. 51m. A.M. on the 2nd the $6\frac{1}{2}$ magnitude star 84 Geminorum disappears at an angle of 184° from the vertex, and reappears at 6h. 12m. A.M. at an angle of 229° from the vertex. On the 7th the $4\frac{1}{2}$ magnitude star ν Virginis disappears at 11h. 6m. P.M. at an angle of 29° from the vertex, and reappears at 0h. 19m. A.M. the next morning at an angle of 219° from the vertex. At 11h. 46m. P.M. on the 10th the 6th magnitude star 94 Virginis approaches the lunar limb at an angle of 297° from the vertex. At 4h. 26m. A.M. on the 15th the 5th magnitude star 4 Sagittarii disappears at an angle of 86° from the vertex, the star being on the horizon at the time, and reappears at 5h. 16m. A.M. at an angle of 190° from the vertex. At 5h. 57m. A.M. the same morning the 6th magnitude star 7 Sagittarii disappears at an angle of 26° from the vertex, and reappears at 6h. 58m. A.M. at an angle of 278° from the vertex, and the $4\frac{1}{2}$ magnitude star 9 Sagittarii disappears at 6h. 31m. A.M. at an angle of 23° from the vertex, and reappears at 7h. 27m. A.M. (11 minutes after sunrise) at an angle of 293° from the vertex.

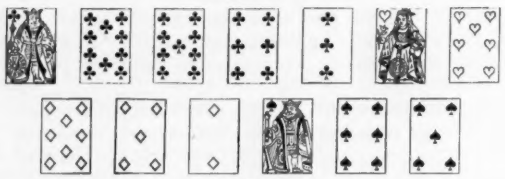
ERRATUM.—In "Face of the Sky for January," page 56, column 1, line 39 from top, for "Schröeter made the diameter 430 miles," read "Schröeter made the diameter 330 miles."

Whist Column.

By W. Montagu Gattie.

THE following hand, which recently occurred in actual play at the Yorick Club, Melbourne, and was published in the *Australasian*, furnishes some very interesting illustrations of the importance of the discard.

Hand No. 9.

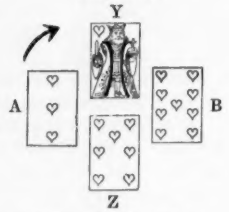


Z's Hand.

Score—Love All. Z turns up the eight of diamonds.

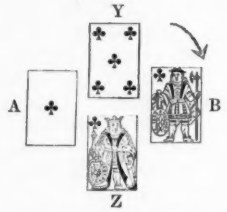
Note.—A and B are partners against Y and Z. A has the first lead; Z is the dealer. The card of the leader to each trick is indicated by an arrow.

Trick 1.



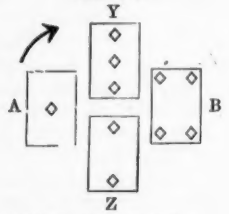
Tricks—AB, 0; YZ, 1.

Trick 2.



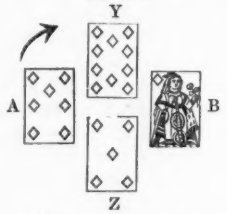
Tricks—AB, 1; YZ, 1.

Trick 3.



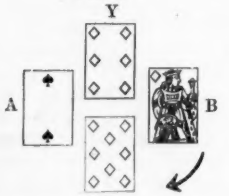
Tricks—AB, 2; YZ, 1.

Trick 4.



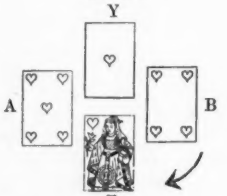
Tricks—AB, 3; YZ, 1.

Trick 5.



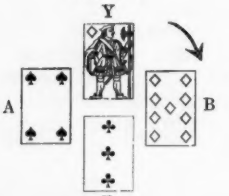
Tricks—AB, 4; YZ, 1.

Trick 6.



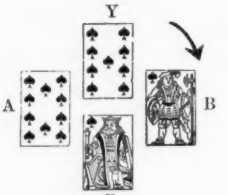
Tricks—AB, 4; YZ, 2.

Trick 7.



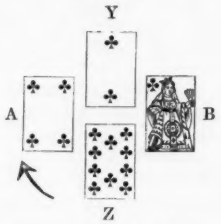
Tricks—AB, 4; YZ, 3.

Trick 8.



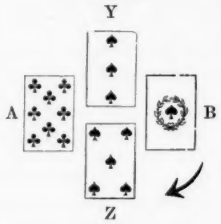
Tricks—AB, 4; YZ, 4.

Trick 9.



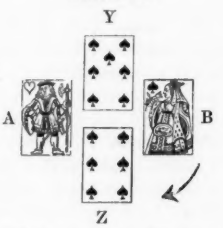
Tricks—AB, 5; YZ, 4.

Trick 10.



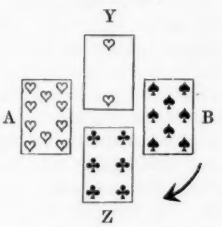
Tricks—AB, 6; YZ, 4.

Trick 11.



Tricks—AB, 7; YZ, 4.

Trick 12.



Tricks—AB, 8; YZ, 4.

Trick 13.—B leads the seven of clubs, on which Z plays the nine, saving the game, and

AB score two by cards and two by honours.

A's Hand.	B's Hand.
D.—Ace, 7.	D.—Kg, Qn, 9, 4.
C.—Ace, 8, 4.	C.—Qn, Kn, 7.
H.—Kn, 10, 8, 5, 3.	H.—9, 4.
S.—10, 4, 2.	S.—Ace, Qn, Kn, 8.

Y's Hand.	Z's Hand.
D.—Kn, 10, 6, 3.	D.—8, 5, 2.
C.—5, 2.	C.—Kg, 10, 9, 6, 3.
H.—Ace, Kg, 6, 2.	H.—Qn, 7.
S.—9, 7, 3.	S.—Kg, 6, 5.

The following remarks are taken from the *Australasian*, our own additions being inclosed in square brackets.

Trick 1.—[Most players would open with the five of hearts.] It is probable that B's nine of hearts is the beginning of a call for trumps. [In our opinion, B's hand does not justify his calling for trumps.]

Trick 2.—On that assumption, Y leads from the weaker of his two weak suits.

Trick 3.—A concludes, and, as the event proves, concludes correctly, that the nine of hearts of trick 1 was the beginning of a call for trumps. [A happens to be right in his guess, but his play is certainly very rash. He should have continued the heart suit. Y, no doubt, after winning with the ace, would have given his partner a force; but, in spite of this, AB should have made two by cards, or one trick more than they ought to have made after leading trumps.]

Trick 4.—It is certainly open to question whether Y's play of the ten of diamonds is defensible. [We believe that Y's play is correct. Z has turned up the eight, and if he also holds the nine, Y's play is immaterial A, of course, cannot hold the nine. If B has it, he may be tempted to finesse against the two cards, and so be enabled to draw all Y's trumps.]

Trick 5.—B's lead of the king is very doubtful policy.

He holds the tenace in the trump suit, and has no means of knowing whether or not there is still a third trump in A's hand. [A's discard is very bad indeed. His partner is clearly void of hearts, so that the ten of spades is really A's only chance of getting in and making his long hearts. Moreover, his clubs are quite worthless, whereas his discard is enough to make his partner imagine that he holds the ten guarded—or at least the nine.]

Trick 6.—This is the trick of the hand. B is void of hearts, and the game is obviously over, if, in addition to the queen of clubs, which is marked in his hand (*see* trick 2), he holds preponderating strength in the spade suit. Therefore Y's best chance seems to be to win Z's queen of hearts, and to draw B's losing trump. Of course, Y takes the risk finding a strong spade in A's hand wherewith to bring in the long hearts; but this risk ought not to prove to be great, because the discard of the spade at trick 5 indicates weakness in the spade suit. [The *coup* is very doubtful policy, and, as a matter of fact, loses a trick. If trick 5 had remained with Z, he would naturally have continued with the ten of clubs; B, winning with queen, would have led out ace and queen of spades; Z, after making the king of spades and nine of clubs, would have continued with another club, which Z would have trumped; and Y would have won the last two tricks with his knave of trumps and ace of hearts, so that YZ would, in all, have made six tricks. In the actual game they made five tricks; but, as will be seen presently, it is doubtful whether, with the best play all round, they should not have lost the game.]

Trick 7.—Z discards the club in order to keep a double guard on the king of spades. But, considering that A has declared weakness in the spade suit, and that Z's clubs will be cleared after another round, Z ought rather to have thrown away the five of spades. In that case Y would have led the two of clubs at the eighth trick, and Z would have been able to make the king of spades and three clubs. [Both discards are wrong. Z should have discarded a spade to indicate his strength in clubs, and A should have thrown away one of his useless clubs.]

Trick 8.—Y, guided by Z's discard of the three of clubs, leads the nine of spades, and Z's king of spades saves the game, the rest of the hand playing itself. The result is that AB, in addition to two by honours, win eight of the tricks, which they would necessarily have done if Y had not covered the queen of hearts. [We dissent from this. See our note to trick 6.] Thus the discard of the club made all the difference between scoring the odd trick and losing two by cards. [But the lead of the two of clubs, in response to a spade discard, might lose the game for YZ, although it would certainly have been the correct play. Let us consider the hand from B's point of view, after his winning the eighth trick with the queen of clubs; and suppose him to reason as follows in favour of leading the eight of spades instead of the ace. If now A has the king of spades, AB must win the game right off. If Y has it, he must, after making it, lead either a spade or a heart, for he has clearly no more clubs. If Z has it, he will probably not put it on, and A will thus have a chance of getting in and making his hearts. Anyhow, nothing would be lost by the venture, for Z must have two spades; so that B must sooner or later make his ace. As the cards actually lie, it will be seen that, unless Z plays his king of spades at once, A wins the trick with the ten and then makes three tricks in hearts, giving AB nine tricks in all. Thus, if B's suggested argument be sound, it would appear that Y's *coup* at trick 6 should properly have resulted in the loss of the game. It is seldom right to win a partner's trick when he has not yet had an opportunity of showing his strong suit; and that proceeding becomes still more difficult to defend, when it involves giving up the command of an adverse suit.]

The usual elementary explanation of the play is necessarily deferred till next month.

Chess Column.

By W. Montagu Gattie.*

[Contributions of general interest to chess-players are invited. Mr. Gunsberg will be pleased to give his opinion on any matter submitted for his decision.]

The French Defence.

That form of the "close game" which consists in the second player's replying to the advance of his antagonist's King's Pawn two squares by moving the opposing Pawn one square only is at least four hundred years old, and is now generally known as the "French Defence." It has been elaborately analysed of late years, and is considered by many authorities, including Major Jaenisch and Mr. Potter, to be the best possible means of neutralizing such advantage as may lie with the first move.

After the opening moves—

White.	Black.
1. P to K4	1. P to K3
2. P to Q4	2. P to Q4

White's best continuation appears to be—

3. Kt to QB3

The old-fashioned move is 3. P × P; but, if White designs to exchange Pawns, he can do so with equal advantage on the following move, while the *sortie* of the Knight gives Black the option of two indifferent replies, viz. 3 . . . B to Kt5 and 3 . . . P × P.

Black's best move is—

3. Kt to KB3

in answer to which White has the choice of three methods of attack, viz. 4. P × P, 4. B to KKt5, and 4. P to K5.

In the first case, the variation best worth remembering is the following:—

4. P × P	4. P × P
5. Kt to B3	5. B to Q3

This move seems correct if Black intends to continue with 6 . . . Castles, and 7 . . . P to B3; but, if he prefers to deploy his Queen's Knight at the seventh move, it is open to question whether King's second is not a better square for the Bishop.

6. B to Q3	6. Castles
7. Castles	7. Kt to B3

If 7 . . . P to B3, the most usual continuation is 8. Kt to K2, Q to B2, 9. Kt to Kt3, B to KKt5 or K3, according to taste, and the game is even.

8. B to KKt5	8. B to KKt5
9. K to Rsq	

If 9. B × Kt, then 9 . . . Q × B, 10. Kt × P, Q to R3, with the advantage.

9. B to K2

If Black play 9 . . . K to Rsq, White wins a Pawn by 10. Kt × P, Kt × P, 11. Kt × Kt, Kt × Kt, 12. P × Kt, P × Kt, 13. B to R6, B to R6, 14. B × R, B × R, 15. B × B, B × B, 16. B × P. There seems nothing better than the text move; and, although the loss of a move is not of so much consequence in openings of this kind as in open games, it must, we think, be conceded that the combination of Black's fifth and seventh moves is theoretically unadvisable.

To return to move 4. If White continues with

4. B to KKt5

the best defence is considered to be

4. B to K2

5. P to K5

Better than 5. B × Kt, B × B, 6. Kt to B3, P to B4 (!)

	5. KKt to Q2
6. B × B	6. Q × B
7. Q to Q2	7. Castles
8. Kt to Qsq	

* Mr. Montagu Gattie has kindly undertaken the chess column while Mr. Gunsberg is in Havannah, where he has gone to play a match with the celebrated Russian player, M. Tschigorin.—Editor.

XUM

If 8. Kt to Kt5, then 8. . . . Kt to Kt3, and Black gains time.

8. P to QB4

The advance of this pawn is an important feature in most variations of the French Defence.

9. P to QB3	9. Kt to QB3
10. P to KB4	10. P × P

We are inclined to prefer this move to 10. . . . P to QKt4.

11. P × P	11. Q to Kt5

and Black is at no disadvantage.

The third course open to White at his fourth move is

4. P to K5

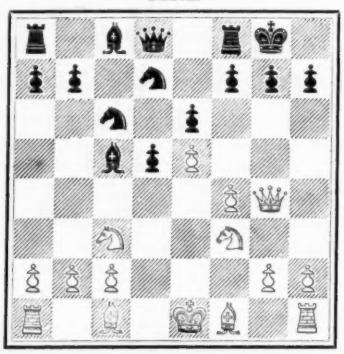
which is favoured by Mr. Steinitz, who carries on the game with

	4. KKt to Q2
5. P to B4	5. P to QB4
6. P × P	6. B × P
7. Q to Kt4	7. Castles

Better on principle, we think, than 7. . . . P to KKt3, which leaves Black's King's Bishop's third square very weak.

8. Kt to B3	8. Kt to QB3

BLACK.



WHITE.

In this position White's best line of attack seems to be

9. B to Q3

threatening to sacrifice the Bishop, and obliging Black to play at once

	9. P to B4
10. Q to R3	10. R to Ksq
11. P to R3	

followed by the advance of the King's Knight's Pawn, with good promise of a lively assault on the Black King.

In the following game, which was played on the 10th of January last, in a match between Messrs. Wainwright and Loman, the latter player adopted a different method of attack after arriving at the position on the diagram. Beginning with that position, the game proceeded as follows:—

WHITE. Mr. R. Loman.	BLACK. Mr. G. E. Wainwright.
9. P to QR3	9. R to Ksq (*a*)
10. B to Q3	10. P to B3
11. Q to R3 (*b*)	11. Kt to Bsq
12. B to Q2	12. B to Q2
13. P to KKt4 (*c*)	13. P to QR3
14. Q to Kt3	14. Q to B2
15. P × P	15. P × P
16. P to Kt5	16. P to B4
17. Castles QR (*d*)	17. P to Kt4
18. P to Kt4 (*e*)	18. B to Q3
19. P to KR4	19. Kt to Kt3 (*f*)
20. Kt to K2	20. P to QR4
21. P to R5	21. KKt to K2
22. P to R6 (*g*)	22. P × P
23. P to Kt6 (*h*)	23. Kt × P
24. Kt to R4	24. Q to R4 (*i*)
25. B to Ksq (*j*)	25. Q × P (ch)
26. K to Q2	26. QKt to K2
27. B to B2	27. P to Kt6 (!)
28. P to B3 (*k*)	28. Q to Kt7 (ch)
29. K to Ksq	29. R to R8
30. B to Q4 (*l*)	30. R × R (ch)
31. K × R	31. R to Rsq (*m*)
32. B to K3	32. R to R8 (ch)
33. Kt to Bsq	33. B to R6 (*n*)
34. Q to Kt5 (*o*)	34. R × Kt (ch)
35. B × R	35. Q × B (ch)
36. K to K2	36. Q × R
37. Q to B6	37. Kt × P (ch)
38. K to K3	38. P to Q5 (ch) (*p*)

And White resigned.

NOTES.

(*a*) It is an interesting question whether White's delay in bringing his Bishop into action permits Black to adopt the system of defence indicated by this and the two following moves, and, since he is not compelled to move his Bishop's pawn two squares, to reserve it for breaking up White's centre.

(*b*) Since the Queen is not attacked by Black's Bishop's Pawn as in the variation given above, this move seems hardly appropriate. It would have been better to proceed with his development.

(*c*) Again a knowledge of the ordinary attack, when Black's Pawn is at Bishop's fourth, induces White to adopt a course that is sarcely so forcible as a rapid development of pieces, or as some plan for driving the adverse King's Bishop from his diagonal (*e.g.* Kt to QR4) followed by castling on the King's side, if possible.

(*d*) Hazardous. We should have preferred to avoid castling altogether in this position, as the King is less in harm's way, and more likely to be useful, in the centre of the board.

(*e*) Unnecessarily courting danger. Kt to K2 at once would have been safer.

(*f*) Mr. Wainwright has so far conducted his defence admirably, and is now in a position to assume the offensive. This move, by drawing off White Queen's Knight, enables the black Pawns on the Queen's flank to push forward without delay.

(*g*) We think that, as White must clearly make some sacrifice in order to maintain his attack, it would here have been better to play 22. P to Kt6, and, if 22... P to R3, then 23. Kt to Kt5.

(*h*) The result of White's long-winded attack is now apparent. He has used his 13th, 14th, 16th, 19th, 21st, and 22nd moves for bringing his Pawns to the front, and now, in order to be beforehand with the attack, he is bound to make a sacrifice.

(*i*) Perhaps better than QKt to K2 at once.

(*j*) Mr. Loman subsequently pointed out that 25. QR to KKtsq would have been a better move.

(*k*) He might have tried 28. R to QRsq, but it is doubtful whether that would have been any better.

(*l*) Threatening to sacrifice his Queen and afterwards to Queen the Rook's Pawn, with some prospects.

(*m*) Black might have won more simply by 31... Q to R8 ch, 32. Kt to Bsq, P to Kt7, &c.

(*n*) Quite conclusive.

(*o*) Vainly hoping to snatch a mate.

(*p*) Very pretty. It is curious that White cannot take the Pawn at all. If 39. Q × P, then 39... B to B4, 40. Q × B, Q to Kt8 ch.

The whole game is beautifully played by Mr. Wainwright.

The solution to the end-game puzzle in our last number is 1 B to K5, B to R8 (best), 2. B × P, B moves, 3. B to Q6, 4. B to B8, and 5. B to Kt7, mate. [If 2. B to Q6, then 2... P to Kt7, &c.]

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